PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

FOURTEENTH MEETING,

HELD AT NEWPORT, RHODE ISLAND,

AUGUST, 1860.

CAMBRIDGE:
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JOSEPH LOVERING,

Permanent Secretary.

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BY ORDER

OF

THE STANDING COMMITTEE,

THE NASHVILLE MEETING OF THE ASSOCIATION,

Appointed for April, 1861,

HAS BEEN POSTPONED FOR ONE YEAR.

OFFICERS OF THE ASSOCIATION

AT THE

NEWPORT MEETING.

ISAAC LEA, LL.D., *President.*
Dr. B. A. GOULD, *Vice-President.*
Prof. JOSEPH LOVERING, *Permanent Secretary.*
Prof. JOSEPH LECONTE, *General Secretary.*
Dr. A. L. ELWYN, *Treasurer.*

Standing Committee.

EX OFFICIO.

ISAAC LEA, LL.D.,
Prof. STEPHEN ALEXANDER,
Prof. EDWARD HITCHCOCK,
Dr. B. A. GOULD,
Prof. JOSEPH LOVERING,
Dr. A. L. ELWYN,
Prof. WILLIAM CHAUVENET,
Prof. JOSEPH LECONTE.

AS CHAIRMAN OF THE SECTIONAL COMMITTEES.

Prof. WOLCOTT GIBBS,
Col. J. W. FORSTER.

FROM THE ASSOCIATION AT LARGE.

Prof. A. D. BACHE,
Prof. J. D. WHITNEY,
Prof. LOUIS AGASSIZ,
Prof. B. SILLIMAN, Jr.,
Prof. JOHN LECONTE,
Prof. W. B. ROGERS.

Local Committee.

Prof. WOLCOTT GIBBS, *Chairman.*
SAMUEL POWEL, Esq., *Secretary.*

Dr. DAVID KING,
Prof. JOSEPH LOVERING,
Prof. HENRY L. EUSTIS,
Prof. ALEXIS CASWELL,
L. M. RUTHERFORD, Esq.,
Prof. N. B. HILL.

*City Committee.**

Mayor WILLIAM H. CRANSTON,
Alderman JOHN C. AILMAN.

MEMBERS OF THE COMMON COUNCIL.

ROBERT J. TAYLOR,
THOMAS COGGESHALL,
JOHN T. BUSH.

* Appointed by the City Government of Newport to coöperate with the Local Committee of Arrangements.

SPECIAL COMMITTEES.

A. Committees continued from former Meetings.

1. *Committee to Report in Relation to Uniform Standards in Weights, Measures, and Coinage.*

Prof. A. D. Bache,	Prof. J. H. Alexander,
Prof. Joseph Henry,	Prof. John F. Frazer,
Prof. Wolcott Gibbs,	Dr. J. H. Gibbon,
Prof. Benjamin Peirce,	Dr. B. A. Gould, Jr.,
Prof. John LeConte,	Prof. J. L. Smith,
Prof. W. B. Rogers,	Prof. R. S. McCulloch.

2. *On the Registration of Births, Deaths, and Marriages.*

Dr. James Wynne,	Dr. F. B. Hough,

E. B. Elliott.

3. *Committee on the Report of the Committee on the Registration of Births, Marriages, and Deaths.*

Prof. Joseph Henry,	Prof. W. M. Gillespie,
Dr. R. W. Gibbes,	Prof. W. B. Rogers,

Prof. Samuel Jackson.

4. *Committee on Dr. I. I. Hayes's Proposed Expedition to the North Pole.*

Prof. A. D. Bache,	Hon. Thomas Ewing,
Prof. Joseph Henry,	Hon. D. M. Barringer,
Prof. W. B. Rogers,	Dr. J. L. LeConte,
Prof. Edward Hitchcock,	Prof. J. E. Hilgard,
Prof. Benjamin Peirce,	Peter Force,
Prof. J. D. Dana,	Dr. Joseph Leidy,
Prof. Joseph Winlock,	Dr. John Torry,

Prof. S. S. Haldeman.

B. New Committees.

1. *Committee to Audit the Accounts of the Permanent Secretary and the Treasurer.*

Dr. B. A. Gould,	Prof. John LeConte.

2. *Committee to act with the Standing Committee in Nomination of Officers for the Nashville Meeting.*

Section A.	Section B.
Prof. L. H. Steiner,	Dr. J. H. Gibbon,
Charles A. Schott,	Dr. R. W. Gibbes,
Prof. J. M. Schofield,	Prof. A. H. Worthen,
Prof. William Chauvenet.	Prof. Daniel Wilson.

OFFICERS OF THE NASHVILLE MEETING.

Pres. F. A. P. BARNARD, *President.*
Dr. R. W. GIBBES, *Vice-President.*
Prof. JOSEPH LOVERING, *Permanent Secretary.*
Prof. W. P. TROWBRIDGE, *General Secretary.*
Dr. A. L. ELWYN, *Treasurer.*

Standing Committee.

Pres. F. A. P. BARNARD,
ISAAC LEA, LL. D.,
Dr. B. A. GOULD,
Dr. R. W. GIBBES,
Prof. JOSEPH LOVERING,
Prof. JOSEPH LECONTE,
Prof. W. P. TROWBRIDGE,
Dr. A. L. ELWYN.

Local Committee.

Dr. J. B. LINDSLEY, *Chairman.*
Dr. B. C. JILLSON, *Secretary.*

Dr. HENRY ERNI,
Dr. PAUL F. EVE,
FRANCIS B. FOGG, Esq.,
Col. A. W. PUTNAM,
RANDALL MCGAVICK,
Dr. R. C. FOSTER, Jr.,
Maj. R. C. MCNAIRY,
HARRY YEATMAN, Esq.

MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Meeting.	Date.	Place.	President.	Vice-President.	General Secretary.	Permanent Secretary.	Treasurer.
1st,	Sept. 20, 1848,	Philadelphia, Pa.,	W.C. Redfield, Esq.,		Prof. Walter R. Johnson,		Prof. J. Wyman.
2d,	Aug. 14, 1849,	Cambridge, Mass.,	Prof. Joseph Henry,		Prof. E. N. Horsford,		Dr. A. L. Elwyn.
3d,	March 12, 1850,	Charleston, S. C.,	Prof. A. D. Bache,*		Prof. L. R. Gibbes,*		Dr. St. J. Ravenel.*
4th,	Aug. 19, 1850,	New Haven, Ct.,	Prof. A. D. Bache,		E. C. Herrick, Esq.,		Dr. A. L. Elwyn.
5th,	May 5, 1851,	Cincinnati, Ohio,	Prof. A. D. Bache,		Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
6th,	Aug. 19, 1851,	Albany, N. Y.,	Prof. L. Agassiz,		Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
7th,	July 28, 1853,	Cleveland, Ohio,	Prof. Benj. Peirce,		Prof. J. D. Dana,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
8th,	April 26, 1854,	Washington, D. C.,	Prof. J. D. Dana,		Prof. J. Lawrence Smith,	Prof. J. Lovering,	Dr. J. L. Le Conte.*
9th,	Aug. 15, 1855,	Providence, R. I.,	Prof. John Torrey,		Prof. Wolcott Gibbs,	Prof. J. Lovering,	Dr. A. L. Elwyn.
10th,	Aug. 20, 1856,	Albany, N. Y.,	Prof. James Hall,		Dr. B. A. Gould,	Prof. J. Lovering,	Dr. A. L. Elwyn.
11th,	Aug. 12, 1857,	Montreal, C. E.,	Prof. J. W. Bailey,	Prof. Alexis Caswell,	Prof. John Leconte,	Prof. J. Lovering,	Dr. A. L. Elwyn.
12th,	April 28, 1858,	Baltimore, Md.,	Prof. A. Caswell,*	Prof. John E. Holbrook,	Prof. W. M. Gillespie,	Prof. J. Lovering,	Dr. A. L. Elwyn.
13th,	Aug. 3, 1859,	Springfield, Mass.,	Prof. S. Alexander,	Prof. Edw. Hitchcock,	Prof. William Chauvenet,	Prof. J. Lovering,	Dr. A. L. Elwyn.
14th,	Aug. 1, 1860,	Newport, R. I.,	Isaac Lea, LL. D.,	Dr. B. A. Gould,	Prof. Joseph Leconte,	Prof. J. Lovering,	Dr. A. L. Elwyn.

* In the absence of the regular officer.

CONSTITUTION OF THE ASSOCIATION.*

OBJECTS.

The Association shall be called "The American Association for the Advancement of Science." The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States; to give a stronger and more general impulse, and a more systematic direction, to scientific research in our country; and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS.

Rule 1. Members of scientific societies or learned bodies having in view any of the objects of this Association, and publishing transactions, shall be considered members on subscribing these rules.

Rule 2. Collegiate professors, also civil engineers and architects who have been employed in the construction or superintendence of public works, may become members on subscribing these rules.

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* Adopted August 25, 1856, and ordered to go into effect at the opening of the Montreal Meeting.

RULE 3. Persons not embraced in the above provisions may become members of the Association upon recommendation in writing by two members, nomination by the Standing Committee, and election by a majority of the members present.

OFFICERS.

RULE 4. The officers of the Association shall be a President, Vice-President, General Secretary, Permanent Secretary, and Treasurer. The President, Vice-President, General Secretary, and Treasurer, shall be elected at each meeting for the following one;— the three first-named officers not to be reëligible for the next two meetings, and the Treasurer to be reëligible as long as the Association may desire. The Permanent Secretary shall be elected at each second meeting, and also be reëligible as long as the Association may desire.

MEETINGS.

RULE 5. The Association shall meet, at such intervals as it may determine, for one week or longer — the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be intrusted to the officers and the Local Committee.

STANDING COMMITTEE.

RULE 6. There shall be a Standing Committee, to consist of the President, Vice-President, Secretaries, and Treasurer of the Association, the officers of the preceding year, the permanent chairman of the Sectional Committees, after these shall have been organized, and six members present from the Association at large, who shall have attended any of the previous meetings, to be elected upon open nomination by ballot on the first assembling of the Association. A majority of the whole number of votes cast to elect. The General Secretary shall be Secretary of the Standing Committee.

The duties of the Standing Committee shall be,—

1. To assign papers to the respective sections.

2. To arrange the scientific business of the general meetings, to suggest topics and arrange the programmes for the evening meetings.

3. To suggest to the Association the place and time of the next meeting.

4. To examine, and, if necessary, to exclude papers.

5. To suggest to the Association subjects for scientific reports and researches.

6. To appoint the Local Committee.

7. To have the general direction of publications.

8. To manage any other general business of the Association during the session, and during the interval between it and the next meeting.

9. In conjunction with four from each Section, to be elected by the Sections for the purpose, to make nominations of officers of the Association for the following meeting.

10. To nominate persons for admission to membership.

11. Before adjourning, to decide which papers, discussions, or other proceedings shall be published.

SECTIONS.

Rule 7. The Association shall be divided into two Sections, and as many sub-Sections as may be necessary for the scientific business, the manner of division to be determined by the Standing Committee of the Association. The two Sections may meet as one.

SECTIONAL OFFICERS AND COMMITTEES.

Rule 8. On the first assembling of the Section, the members shall elect upon open nomination a permanent chairman and secretary, also three other members, to constitute, with these officers, a Sectional Committee.

The Section shall appoint, from day to day, a chairman to preside over its meetings.

RULE 9. It shall be the duty of the Sectional Committee of each Section to arrange and direct the proceedings in their Section; to ascertain what communications are offered; to assign the order in which these communications shall appear, and the amount of time which each shall occupy.

The Sectional Committees may likewise recommend subjects for systematic investigation by members willing to undertake the researches, and to present their results at the next meeting.

The Sectional Committees may likewise recommend reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent meetings.

REPORTS OF PROCEEDINGS.

RULE 10. Whenever practicable, the proceedings shall be reported by professional reporters or stenographers, whose reports are to be revised by the secretaries before they appear in print.

PAPERS AND COMMUNICATIONS.

RULE 11. No paper shall be placed in the programme, unless admitted by the Sectional Committee; nor shall any be read, unless an abstract of it has been previously presented to the Secretary of the Section, who shall furnish to the chairman the titles of papers, of which abstracts have been received.

RULE 12. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declares such to be his wish before presenting it to the Association.

RULE 13. Copies of all communications, made either to the General Association or to the Sections, must be furnished by the authors; otherwise only the titles or abstracts shall appear in the published proceedings.

RULE 14. All papers, either at the general or in the sectional meetings, shall be read, as far as practicable, in the order in which they are entered upon the books of the Association; except that those which may be entered by a member of the Standing Committee of the Association shall be liable to postponement by the proper Sectional Committee.

RULE 15. If any communication be not ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

RULE 16. No exchanges shall be made between members without authority of the respective Sectional Committees.

GENERAL AND EVENING MEETINGS.

RULE 17. The Standing Committee shall appoint any general meeting which the objects and interests of the Association may call for, and the evenings shall, as a rule, be reserved for general meetings of the Association.

These general meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Sections.

It shall be a part of the business of these general meetings to receive the Address of the President of the last meeting; to hear such reports on scientific subjects as, from their general importance and interests, the Standing Committee shall select; also to receive from the chairman of the Sections abstracts of the proceedings of their respective Sections; and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

RULE 18. The Association shall be called to order by the President of the preceding meeting, and this officer having resigned the chair to the President elect, the General Secretary shall then report the number of papers relating to each department which have been registered, and the Association consider the most eligible distribution into Sections, when it shall proceed to the election of the additional members of the Standing Committee in the manner before described; the meeting shall then adjourn, and the Standing Committee, having divided the Association into Sections as directed, shall allot to each its place of meeting for the Session. The Sections shall then organize by electing their officers and their representatives in the Nominating Committee, and shall proceed to business.

PERMANENT SECRETARY.

RULE 19. It shall be the duty of the Permanent Secretary to notify members who are in arrears, to provide the necessary stationery and suitable books for the list of members and titles of papers, minutes of the general and sectional meetings, and for other purposes indicated in the rules, and to execute such other duties as may be directed by the Standing Committee or by the Association.

The Permanent Secretary shall make a report annually to the Standing Committee, at its first meeting, to be laid before the Association, of the business of which he has had charge since its last meeting.

All members are particularly desired to forward to the Permanent Secretary, so as to be received before the day appointed for the Association to convene, complete titles of all the papers which they expect to present during its meeting, with an estimate of the time required for reading each, and such abstracts of their contents as may give a general idea of their nature.

Whenever the Permanent Secretary notices any error of fact or unnecessary repetition, or any other important defect in the papers communicated for publication in the proceedings of the Association, he is authorized to commit the same to the author, or to the proper sub-committee of the Standing Committee for correction.

LOCAL COMMITTEE.

RULE 20. The Local Committee shall be appointed from among members residing at or near the place of meeting for the ensuing year; and it shall be the duty of the Local Committee, assisted by the officers, to make arrangements and the necessary announcements for the meeting.

The Secretary of the Local Committee shall issue a circular in regard to the time and place of meetings, and other particulars, at least one month before each meeting.

SUBSCRIPTIONS.

RULE 21. The amount of the subscription, at each meeting, of each member of the Association shall be two dollars, and one dollar in addition shall entitle him to a copy of the proceedings of the annual meeting. These subscriptions shall be received by the Permanent Secretary, who shall pay them over, after the meeting, to the Treasurer.

No person shall be considered a member of the Association until the subscription for the meeting at which he is elected has been paid.

RULE 22. The names of all persons two years in arrears for annual dues shall be erased from the list of members; provided that two notices of indebtedness, at an interval of at least three months, shall have been previously given.

ACCOUNTS.

Rule 23. The accounts of the Association shall be audited annually, by auditors appointed at each meeting.

ALTERATIONS OF THE CONSTITUTION.

Rule 24. No article of this constitution shall be altered, or amended, or set aside, without the concurrence of three fourths of the members present, and unless notice of the proposed change shall have been given at the preceding annual meeting.

RESOLUTIONS

OF A

PERMANENT AND PROSPECTIVE CHARACTER,

ADOPTED AUGUST 19, 1857.

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1. No appointment may be made in behalf of the Association, and no invitation given or accepted, except by vote of the Association or its Standing Committee.

2. The General Secretary shall transmit to the Permanent Secretary for the files, within two weeks after the adjournment of every meeting, a record of the proceedings of the Association and the votes of the Standing Committee. He shall also daily, during the meetings, provide the chairman of the two sectional committees with lists of the papers assigned to their Sections by the Standing Committee.

3. All printing for the Association shall be superintended by the Permanent Secretary, who is authorized to employ a clerk for that especial purpose.

4. The Permanent Secretary is authorized to put the proceedings of the meetings to press one month after the adjournment of the Association. Papers which have not been received at that time may be published only by title. No notice of articles not approved shall be taken in the published proceedings.

5. The Permanent Chairmen of the Sections are to be considered their organs of communication with the Standing Committee.

6. It shall be the duty of the Secretaries of the two Sections to receive copies of the papers read in their Sections, all sub-sections included, and to furnish them to the Permanent Secretary at the close of the meeting.

7. The Sectional Committees shall meet not later than 9 A. M. daily during the meetings of the Association, to arrange the programmes of their respective sections, including all sub-sections, for the following day. No paper shall be placed upon these programmes which shall not have been assigned to the Section by the Standing Committee. The programmes are to be furnished to the Permanent Secretary not later than 11 A. M.

8. During the meetings of the Association the Standing Committee shall meet daily, Sundays excepted, at 9 A. M., and the Sections be called to order at 10 A. M., unless otherwise ordered. The Standing Committee shall also meet on the evening preceding the first assembling of the Association at each annual meeting, to arrange for the business of the first day, and on this occasion three shall form a quorum.

9. Associate members may be admitted for one, two, or three years, as they shall choose at the time of admission, — to be elected in the same way as permanent members, and to pay the same dues. They shall have all the social and scientific privileges of members, without taking part in the business.

10. No member may take part in the organization and business arrangement of both the Sections.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

NOTE. — Names of deceased members are marked with an asterisk (*). The figure at the end of each name refers to the meeting at which the election took place.

A.

Abbott, Gorham D., New York, New York [7].
Abernethy, William, Oregon City, California [12].
*Adams, Prof. C. B., Amherst, Massachusetts [1].
Aiken, Prof. W. E. A., Baltimore, Maryland [12].
Ainsworth, J. G., Barry, Massachusetts [14].
Albert, Augustus J., Baltimore, Maryland [12].
Alexander, Prof. Stephen, Princeton, New Jersey [1].
Allen, Prof. E. A. H., New Bedford, Massachusetts [6].
Allen, Zachariah, Providence, Rhode Island [1].
Allston, R. F. W., Georgetown, South Carolina [3].
*Ames, M. P., Springfield, Massachusetts [1].
Andrews, Prof. E. B., Marietta, Ohio [7].
Anthony, Charles H., Albany, New York [6].
Appleton, Nathan, Boston, Massachusetts [1].

Atkinson, Dr. Robert, Baltimore, Maryland [12].
Atterbury, Rev. John G., New Albany, Indiana [11].
Avery, Prof. Charles, Clinton, New York [10].

B.

Bache, Prof. Alexander D., Washington, District of Columbia [1].
Bache, Dr. Franklin, Philadelphia, Pennsylvania [1].
Bacon, Dr. John, Jr., Boston, Massachusetts [1].
Bagg, Dr. Moses M., Utica, New York [4].
*Bailey, Prof. J. W., West Point, New York [1].
Baird, Prof. S. F., Washington, District of Columbia [1].
Baird, Dr. Thomas D., Baltimore, Maryland [12].
Baldwin, F. H., Waverly, New York [10].
Bardwell, Prof. F. W., Yellow Springs, Ohio [13].
Barker, George F., Boston, Massachusetts [13].
Barlow, Thomas, Canastota, New York [7].
Barnard, Pres. F. A. P., Oxford, Mississippi [7].
Barnard, Henry, Madison, Wisconsin [12].
Barnard, J. G., U. S. A., Washington, District of Columbia [14].
Barnes, Capt. James, Springfield, Massachusetts [5].
Barratt, Dr. Joseph, Middletown, Connecticut [13].
Barringer, Hon. D. M., Charlotte, North Carolina [12].
Barry, L. F., Baltimore, Maryland [12].
Bartlett, Prof. W. H. C., West Point, New York [9].
Basnett, Thomas, Ottawa, Illinois [8].
Batchelder, J. M., Cambridge, Massachusetts [8].
Baylis, James, Montreal, Canada [11].
Beadle, Dr. E. L., New York, New York [1].
Beadle, Rev. E. R., Hartford, Connecticut [10].
Bebb, M. S., Rockford, Illinois [13].
*Beck, Dr. C. F., Philadelphia, Pennsylvania [1].
*Beck, Prof. Lewis C., New Brunswick, New Jersey [1].
*Beck, Dr. T. Romeyn, Albany, New York [1].
Bell, Samuel N., Manchester, New Hampshire [7].
Benedict, Dr. N. B., New Orleans, Louisiana [10].
Benedict, Thomas B., Kirk's Ferry, Louisiana [11].
Bentley, Cyrus, Chicago, Illinois [13].

Berdon, Hiram, New York, New York [12].
Berezy, William, Daillehaut, Canada East [11].
Bidwell, Walter H., New York, New York [11].
Bigelow, Artemas, Newark, New Jersey [9].
*Binney, Dr. Amos, Boston, Massachusetts [1].
*Binney, John, Boston, Massachusetts [3].
Blackie, Dr. George S., Nashville, Tennessee [10].
Blair, Prof. F. O., Lebanon, Illinois [13].
Blake, Eli W., New Haven, Connecticut [1].
Blake, J. R., La Grange, Tennessee [10].
Blake, William P., Washington, District of Columbia [2].
*Blanding, Dr. William, Rhode Island [1].
Blaney, Prof. James V. Z., Chicago, Illinois [12].
Bledsoe, Prof. A. T., University of Virginia, Virginia [12].
Bolton, James, Richmond, Virginia [10].
*Bomford, Col. George, Washington, District of Columbia [1].
Botta, Prof. Vincenzo, New York, New York [9].
Bouve, Thomas T., Boston, Massachusetts [1].
Bowditch, Dr. Henry J., Boston, Massachusetts [2].
Bowman, Francis C., New York, New York [12].
Boynton, Prof. Edward C., Oxford, Mississippi [13].
Bradford, Isaac, Cambridge, Massachusetts [14].
Brevoort, J. Carson, Brooklyn, New York [1].
Brewer, Fisk P., New Haven, Connecticut [11].
Briggs, A. D., Springfield, Massachusetts [13].
Brocklesby, Prof. John, Hartford, Connecticut [4].
Bross, William, Chicago, Illinois [7].
Brown, Andrew, Natchez, Mississippi [1].
Brown, Richard, Sydney, Cape Breton [1].
Brown, Robert, Jr., Cincinnati, Ohio [11].
Brunnow, Prof. F., Ann Arbor, Michigan [10].
Brush, George J., New Haven, Connecticut [11].
Buchanan, Robert, Cincinnati, Ohio [2].
*Burnap, Rev. G. W., Baltimore, Maryland [12].
*Burnett, Waldo I., Boston, Massachusetts [1].
Busher, James, Worcester, Massachusetts [9].
Butler, Prof. James D., Madison, Wisconsin [13].
Butler, Hon. Thomas B., Norwalk, Connecticut [10].

C.

Cabell, Prof. James L., University of Virginia, Virginia [6].
Carpenter, Prof. J. N., Washington, District of Columbia [12].
*Carpenter, Thornton, Camden, South Carolina [7].
*Carpenter, Dr. William M., New Orleans, Louisiana [1].
Carr, E. S., Albany, New York [9].
Case, Hon. William, Cleveland, Ohio [6].
Cassells, Prof. J. L., Cleveland, Ohio [7].
Caswell, Prof. Alexis, Providence, Rhode Island [2].
Chadbourne, Prof. P. A., Williamstown, Massachusetts [10].
Chapin, A. L., Beloit, Wisconsin [14].
Chapin, L. C., New Haven, Connecticut [11].
*Chapman, Dr. N., Philadelphia, Pennsylvania [1].
Chapman, Prof. C. B., Madison, Wisconsin [11].
Chase, Prof. George I., Providence, Rhode Island [1].
*Chase, Prof. S., Dartmouth, New Hampshire [2].
Chauvenet, Prof. William, St. Louis, Missouri [1].
Chesbrough, E. S., Chicago, Illinois [2].
Chittenden, L. E., Burlington, Vermont [14].
Church, Prof. A. E., West Point, New York [10].
Churchill, Marlborough, Sing Sing, New York [13].
Clapp, Dr. Asahel, New Albany, Indiana [1].
Clark, Alvin, Cambridgeport, Massachusetts [4].
Clark, H. J., Cambridge, Massachusetts [13].
Clark, J. H., Philadelphia, Pennsylvania [12].
*Clark, Joseph, Cincinnati, Ohio [5].
Clark, M. Lewis, St. Louis, Missouri [5].
Clark, Prof. James, Georgetown, District of Columbia [8].
Clarke, Dr. A. B., Holyoke, Massachusetts [13].
Clarke, Henry, M. D., Worcester, Massachusetts [14].
Cleaveland, Prof. C. H., Cincinnati, Ohio [9].
*Cleveland, Dr. A. B., Cambridge, Massachusetts [2].
Clum, Henry A., New York, New York [9].
Coakley, Prof. George W., New York, New York [5].
Cobleigh, Pres. Nelson E., Lebanon, Illinois [13].
Coffin, C. C., Malden, Massachusetts [13].
Coffin, Prof. James H., Easton, Pennsylvania [1].

Coffin, Prof. John H. C., Annapolis, Maryland [1].
*Cole, Thomas, Salem, Massachusetts [1].
*Coleman, Rev. Henry, Boston, Massachusetts [1].
Comstock, C. B., West Point, New York [14].
Conant, Marshall, South Bridgewater, Massachusetts [7].
Congdon, Charles, New York, New York [1].
Conkling, Frederick A., New York, New York [11].
Conway, Rev. M. D., Cincinnati, Ohio [14].
Cook, Prof. George H., New Brunswick, New Jersey [6].
Copes, Dr. Joseph S., New Orleans, Louisiana [11].
Corning, Hon. Erastus, Albany, New York [6].
Coryell, Thomas D., Madison, Wisconsin [13].
Cottle, Thomas J., Woodstock, Upper Canada [10].
Couper, J. Hamilton, Darien, Georgia [1].
Cox, Dr. Christopher C., Easton, Maryland [12].
Craik, Dr. Robert, Montreal, Canada [11].
Cramp, Rev. J. M., Acadia College, Nova Scotia [11].
Crosby, Alpheus, Salem, Massachusetts [10].
Cummings, Pres. Joseph, Middletown, Connecticut [13].
Curley, Prof. James, Georgetown, District of Columbia [8].
Curry, Rev. W. F., Geneva, New York [11].
Curtiss, Charles W., Evanston, Illinois [13].

D.

Dakin, Francis E., Freeport, Illinois [10].
Dalrymple, Rev. E. A., Baltimore, Maryland [11].
Dana, Prof. James D., New Haven, Connecticut [1].
Danforth, Edward, Grand Rapids, Michigan [11].
Dascomb, Prof. James, Oberlin, Ohio [7].
Davies, Prof. Charles, Fishkill, New York [10].
Davies, W. H. A., Montreal, Canada [11].
Davis, James, Jr., Boston, Massachusetts [1].
Dawson, Prof. J. W., Montreal, Canada [10].
Dean, Prof. Amos, Albany, New York [6].
*Dearborn, George H. A. S., Roxbury, Massachusetts [1].
*DeKay, Dr. James E., New York, New York [1].
Delafield, Joseph, New York, New York [1].

Delano, Joseph C., New Bedford, Massachusetts [5].
Denson, Claudius B., Duplin Co., North Carolina [12].
Dewey, Prof. Chester, Rochester, New York [1].
Dexter, James, Albany, New York [6].
Dexter, G. M., Boston, Massachusetts [11].
Dickson, Rev. Cyrus, Baltimore, Maryland [12].
Dinwiddie, Robert, New York, New York [1].
Dixwell, Epes S., Cambridge, Massachusetts [1].
Donaldson, Dr. Francis, Baltimore, Maryland [12].
Doremus, R. Ogden, New York, New York [10].
Dow, Prof. George W., Chicago, Illinois [13].
Downes, John, Washington, District of Columbia [10].
Drowne, Prof. Charles, Troy, New York [6].
*Ducatel, Dr. J. T., Baltimore, Maryland [1].
Duffield, Rev. George, Detroit, Michigan [10].
Dumont, Rev. A. H., Newport, Rhode Island [14].
*Duncan, Lucius C., New Orleans, Louisiana [10].
Dunglison, Prof. Robley, Philadelphia, Pennsylvania [8].
Dunn, Prof. R. P., Providence, Rhode Island [14].
Dunn, T. C., M. D., Newport, Rhode Island [14].
Dupuy, Prof. Charles H., Baltimore, Maryland [12].
Dwinelle, John H., Rochester, New York [11].
Dwinelle, William H., New York, New York [10].
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Easton, Norman, Fall River, Massachusetts [14].
Eastwood, George, Saxonville, Massachusetts [13].
Eaton, Daniel C., New Haven, Connecticut [13].
Eggleston, Rev. N. H., Madison, Wisconsin [13].
Eliot, Prof. Charles W., Cambridge, Massachusetts [14].
Elliott, Ezekiel B., Boston, Massachusetts [10].
Elwyn, Dr. Alfred L., Philadelphia, Pennsylvania [1].
Ely, George H., Rochester, New York [13].
Emerson, George B., Boston, Massachusetts [1].
Engelmann, Dr. George, St. Louis, Missouri [1].

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Eustis, Prof. Henry L., Cambridge, Massachusetts [2].
Eve, Prof. Paul F., Nashville, Tennessee [12].
Everett, Edward, Boston, Massachusetts [2].
Everett, J. D., Winsor, Nova Scotia [14].
Ewing, Thomas, Lancaster, Ohio [5].

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Farmer, Moses G., Boston, Massachusetts [9].
Farquhar, W. H., Fair Hill, Montgomerie Co., Maryland [12].
Ferrell, William, Nashville, Tennessee [11].
Ferris, Rev. Dr. Isaac, New York, New York [6].
Feuchtwanger, Dr. Louis, New York, New York [11].
Field, Roswell, Greenfield, Massachusetts [13].
Fillmore, Millard, Buffalo, New York [7].
Fisher, Mark, Trenton, New Jersey [10].
Fitch, Edward H., Ashtabula, Ohio [11].
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Folsom, George, New York, New York [11].
Foote, Elisha, Seneca Falls, New York [10].
Force, Dr. Charles F., Washington, District of Columbia [12].
Force, Col. Peter, Washington, District of Columbia [4].
Ford, Richard, Columbia, South Carolina [12].
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Foster, J. W., Chicago, Illinois [1].
Fowle, William B., Boston, Massachusetts [1].
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French, J. H., Syracuse, New York [11].
Fristoe, Edward T., Washington, District of Columbia [11].
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Kite, Thomas, Cincinnati, Ohio [5].
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Kloman, Dr. W. C., Baltimore, Maryland [12].
Knox, Rev. J. P., Newtown, Long Island [11].
Kurtz, J. D., Portland, Maine [3].

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Lawrence, Prof. Edward A., East Windsor, Connecticut [13].
Lawrence, George N., New York, New York [7].
Lea, Isaac, Philadelphia, Pennsylvania [1].
Lea, Robert M., Aransas, Texas [12].
Leckie, Robert, Montreal, Canada [11].
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Lesley, Joseph, Jr., Philadelphia, Pennsylvania [8].
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Lincklaen, Ledyard, Cazenovia, New York, New York [1].
Lincoln, Dr. N. S., Washington, District of Columbia [12].
Lindsley, Dr. J. B., Nashville, Tennessee [1].
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Litton, Prof. A., St. Louis, Missouri [12].
Locke, Prof. Joseph M., Cincinnati, Ohio [13].
Locke, Dr. Luther F., Nashua, New Hampshire [7].
Logan, Sir William E., Montreal, Canada [1].
Loomis, Prof. Elias, New Haven, Connecticut [1].
Loomis, Prof. J. R., Lewisburg, Pennsylvania [10].
Loomis, Prof. L. C., Washington, District of Columbia [9].
Loomis, Dr. Silas L., Washington, District of Columbia [7].
Lord, Rev. John, Stamford, Connecticut [13].
Loosey, Charles F., New York, New York [12].
Lovering, Prof. Joseph, Cambridge, Massachusetts [2].
Lunn, William, Montreal, Canada [11].
Lyman, Prof. Chester S., New Haven, Connecticut [4].
Lyman, Henry, Montreal, Canada [11].
Lynch, Rev. Dr. P. N., Charleston, South Carolina [2].
Lyon, Caleb, Lyonsdale, New York [8].

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M'Cabe, Rev. James D., Mount Washington, Maryland [12].
McCall, John, Utica, New York [10].
McCauley, James, Elkton, Cecil Co., Maryland [12].
McChesney, J. H., Springfield, Illinois [11].
M'Conihe, Isaac, Troy, New York [4].
McCord, J. L., Montreal, Canada [11].
McCron, Rev. John, Baltimore, Maryland [12].
McFarlan, Henry, Dover, New Jersey [8].
McKew, Dr. Dennis, Baltimore, Maryland [12].
McRae, John, Camden, South Carolina [3].
Maclean, Prof. George M., Pittsburg, Pennsylvania [5].
Macy, Alfred, Nantucket, Massachusetts [13].
Mahan, Prof. D. H., West Point, New York [9].
Mallett, Prof. J. W., Tuscaloosa, Alabama [10].
Marcy, Oliver, Wilbraham, Massachusetts [10].
*Marsh, Dexter, Esq., Greenfield, Massachusetts [1].
Martin, Prof. W. J., Chapel Hill, North Carolina [12].
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Maupin, Prof. S., Charlottesville, Virginia [10].
Mauran, Dr. J., Providence, Rhode Island [2].
Mayhew, Prof. D. P., Ypsilanti, Michigan [13].
Means, Prof. A., Oxford, Georgia [5].
Meech, L. W., Preston, Connecticut [8].
Meek, F. B., Albany, New York [6].
Meigs, Prof. James A., Philadelphia, Pennsylvania [12].
Meilleur, Dr. J. B., Montreal, Canada [11].
Merrill, Hubert H., Lebanon, Tennessee [13].
Merrill, Stephen, Charlestown, Massachusetts [11].
Merryman, Dr. M., Baltimore, Maryland [12].
Meyers, Prof. A. M., Baltimore, Maryland [12].
Miles, Prof. Henry H., Lennoxville, Canada East [11].
Miller, Samuel, New Haven, Connecticut [14].
Minifie, William, Baltimore, Maryland [12].
Mitchell, Henry, Nantucket, Massachusetts [10].
Mitchell, Maria, Nantucket, Massachusetts [4].
Mitchell, William, Nantucket, Massachusetts [2].
Mittag, J. F. G., Hagerstown, Maryland [12].
Moffatt, George, Montreal, Canada [11].
Moore, George H., New York, New York [8].
Montague, Theodore L., Pomeroy, Ohio [13].
Morgan, DeWitt C., M. D., Baltimore, Maryland [14].
Morgan, Lewis H., Rochester, New York [10].
Morris, D., Ellingham, Connecticut [14].
Morris, J. R., Houston, Texas [11].
Morris, Rev. John G., Baltimore, Maryland [12].
Morrison, Benjamin F., Nantucket, Massachusetts [13].
Morrow, E. G., Chapel Hill, North Carolina [11].
Morse, Charles M., Waterville, Maine [11].
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Munger, George G., Rochester, New York [10].
Munroe, Rev. Nathan, Bradford, Massachusetts [6].
Murdoch, Charles N., Baltimore, Maryland [12].
Murdock, John, Baltimore, Maryland [12].
Murray, Prof. David, Albany, New York [11].
Myers, Gustavus A., Richmond, Virginia [11].

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Nelson, Rev. Cleland K., Annapolis, Maryland [12].
Newcomb, Simon, Cambridge, Massachusetts [13].
Newton, Prof. Hubert A., New Haven, Connecticut [6].
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Nolen, George A., New Haven, Connecticut [13].
Norton, Edward, Farmington, Connecticut [13].
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Ordway, John M., Providence, Rhode Island [9].
Ormand, J. J., Tuscaloosa, Alabama [10].
Osgood, Rev. Samuel, New York, New York [8].
Osten, Sacken, Baron R. von, Washington, D. C. [10].
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Parker, Prof. William H., Middlebury, Vermont [10].
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Parry, Dr. Charles C., Davenport, Iowa [6].
Parvin, Theodore S., Iowa City, Iowa [7].
Patterson, Prof. J. W., Hanover, New Hampshire [11].
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Peck, Prof. William G., New York, New York [13].
Peckham, Dr. F. H., Providence, Rhode Island [12].
Peirce, Prof. Benjamin, Cambridge, Massachusetts [1].
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Perkins, Prof. George R., Utica, New York [1].
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Peters, Dr. C. H. F., Clinton, New York [9].
Phelps, Almira L., Baltimore, Maryland [13].
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Pierrepont, H. E., Brooklyn, New York [14].
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Pope, Prof. Charles A., St. Louis, Missouri [12].
Porter, Prof. Charles H., Albany, New York [13].
Porter, Charles T., New York, New York [13].
Porter, Prof. John A., New Haven, Connecticut [14].
Porter, Prof. Thomas C., Lancaster, Pennsylvania [12].
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Resor, Jacob, Cincinnati, Ohio [8].
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Rogers, Prof. Fairman, Philadelphia, Pennsylvania [11].
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Sands, Samuel, Baltimore, Maryland [12].
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Sutherland, Prof. William, Montreal, Canada [6].
Swallow, Prof. G. C., Columbia, Missouri [10].
Swinburne, John, Albany, New York [6].

T.

*Tallmadge, Hon. James, New York, New York [1].
Tatlock, Prof. John, Williamstown, Massachusetts [10].
*Taylor, Richard C., Philadelphia, Pennsylvania [1].
Taylor, W. I., Worcester Co., Maryland [12].

*Teschemacher, J. E., Boston, Massachusetts [1].
Thickstun, T. F., Chatfield, Minnesota [11].
Thomas, William A., Kingston, Massachusetts [10].
Thompson, Dr. Alexander, Aurora, New York [6].
Thompson, Aaron R., New York, New York [1].
Thompson, H. C., Chapel Hill, North Carolina [13].
*Thompson, Rev. Z., Burlington, Vermont [1].
Thurber, Isaac, Providence, Rhode Island [9].
Tilghman, Gen. Tench, Oxford, Maryland [12].
Tingley, Prof. Joseph, Greencastle, Indiana [14].
Tobey, Dr. Samuel B., Providence, Rhode Island [9].
Tolderoy, Dr. James B., Fredericton, New Brunswick [11].
Torrey, Dr. John, New York, New York [1].
Totten, Gen. J. G., U. S. A., Washington, D. C. [1].
Townsend, Hon. Franklin, Albany, New York [4].
*Townsend, John K., Philadelphia, Pennsylvania [1].
Townsend, Robert, Albany, New York [9].
Treadwell, C. P., L'Original, Canada West [11].
*Troost, Dr. Gerard, Nashville, Tennessee [1].
Trowbridge, Prof. W. P., Washington, District of Columbia [10].
*Tuomey, Prof. M., Tuscaloosa, Alabama [1].
Turner, Dr. Henry E., Newport, Rhode Island [14].
Tuthill, Dr. Franklin, New York, New York [8].
Tuttle, David K., University of Virginia [12].
*Tyler, Rev. Edward R., New Haven, Connecticut [1].
Tyler, P. B., Springfield, Massachusetts [13].
Tyler, Ransom H., Fulton, New York [10].
Tyson, Philip T., Baltimore, Maryland [12].

U.

Uhler, P. R., Baltimore, Maryland [12].
Upham, G. B., M. D., Boston, Massachusetts [14].

V.

Vail, Prof. Hugh, Philadelphia, Pennsylvania [8].
Van Benschoten, James C., Oxford, Chenango Co., New York [12].

*Vancleve, John W., Dayton, Ohio [1].
*Vanuxem, Lardner, Bristol, Pennsylvania [1].
Van Vleck, J. M., Middletown, Connecticut [9].
Vaux, William S., Philadelphia, Pennsylvania [1].

W.

Wadsworth, Charles F., Genesee, New York [11].
Wadsworth, James S., Genesee, New York [2].
Wagner, Tobias, Philadelphia, Pennsylvania [9].
Walker, Joseph, Oxford, New York [10].
*Walker, Sears C., Washington, D. C. [1].
*Walker, Hon. Timothy, Cincinnati, Ohio [4].
Ward, Henry A., Rochester, New York [13].
Warner, H. G., Rochester, New York [11].
Warren, G. K., U. S. A., Washington, D. C. [12].
*Warren, Dr. John C., Boston, Massachusetts [1].
Watson, Prof. James C., Ann Arbor, Michigan [13].
Watson, William, Cambridge, Massachusetts [12].
*Webster, H. B., Albany, New York [1].
*Webster, Dr. J. W., Cambridge, Massachusetts [1].
*Webster, M. H., Albany, New York [1].
Webster, Nathan B., Portsmouth, Virginia [7].
Welch, John, Newark, New Jersey [10].
West, Charles E., Buffalo, New York [1].
Wethered, Charles E., Baltimore, Maryland [12].
Wheatland, Dr. Henry, Salem, Massachusetts [1].
Wheatland, Richard H., Salem, Massachusetts [13].
Wheatley, Charles M., New York, New York [1].
Wheeler, Dr. T. B., Montreal, Canada [11].
Wheildon, W. W., Charlestown, Massachusetts [13].
White, Charles, Crawfordsville, Indiana [10].
White, Prof. Henry H., Harrodsburg, Kentucky [14].
Whitney, Asa, Philadelphia, Pennsylvania [1].
Whitney, H. H., Montreal, Canada [11].
Whitney, J. D., Northampton, Massachusetts [1].
Whitney, Prof. William D., New Haven, Connecticut [12].
Whittlesey, Charles, Cleveland, Ohio [1].

Whittlesey, Charles C., St. Louis, Missouri [11].
Wilkes, Capt. Charles, U. S. N., Washington, D. C. [1].
Williams, Dr. Henry W., Boston, Massachusetts [11].
Williams, Matthew, M. D., Syracuse, New York [13].
Williams, Thomas H., Portsmouth, Virginia [12].
Williams, William W., New York, New York [12].
Williamson, Lieut. R. S., U. S. A., San Francisco, Cal. [12].
Wilson, Prof. Daniel, Toronto, Canada [10].
Wilson, Prof. W. C., Carlisle, Pennsylvania [12].
Woodbridge, George A., Nashville, Tennessee [10].
*Woodbury, Hon. L., Portsmouth, New Hampshire [1].
Woodman, John S., Hanover, New Hampshire [11].
Worthen, A. H., Springfield, Illinois [5].
Wright, A. W., New Haven, Connecticut [14].
Wright, Chauncey, Cambridge, Massachusetts [9].
*Wright, Dr. John, Troy, New York [1].
Wurtele, Rev. Louis C., Lennoxville, Canada East [11].
Wurtz, Prof. Henry, Washington, District of Columbia [10].
Wynne, Thomas H., Richmond, Virginia [8].
Wynne, Dr. James, New York, New York [14].

Y.

Youmans, E. L., Saratoga Springs, New York [6].
*Young, Prof. Ira, Hanover, New Hampshire [7].
Young, William H., Baltimore, Maryland [12].

The above list contains seven hundred and twenty-six names, of which eighty-two are of deceased members.

MEMBERS ELECTED

AT

THE NEWPORT MEETING.

‡Ainsworth, J. G., Barry, Massachusetts.
‡Barnard, J. G., U. S. A., Washington, District of Columbia.
†Bell, Sanford, M. D., Springfield, Illinois.
‡Bradford, Isaac, Cambridge, Massachusetts.
*Chapin, Pres. A. L., Beloit, Wisconsin.
Chittenden, L. E. [A], Burlington, Vermont.
Clarke, Henry, M. D., Worcester, Massachusetts.
Comstock, C. B., U. S. A., West Point, New York.
‡Conway, Rev. M. D., Cincinnati, Ohio.
†Craft, Rev. David [A], Asylum, Bradford Co., Pennsylvania.
†Cullum, Capt. G. W., U. S. A., Newport, Rhode Island.
‡Dumont, Rev. A. H. [A], Newport, Rhode Island.
*Dunn, Prof. R. P., Providence, Rhode Island.
Dunn, Dr. T. C., Newport, Rhode Island.
‡Easton, Norman, Fall River, Massachusetts.
*Eliot, Prof. Charles W., Cambridge, Massachusetts.
*Everett, J. D., Windsor, Nova Scotia.
*Fairbanks, Prof. Henry, Hanover, New Hampshire.
Hammond, George T. [A], Newport, Rhode Island.

Those marked thus [A] were chosen Associate Members.

* Those marked with an asterisk paid the assessment and signed the Constitution, without being formally elected.

† Have not been heard from.

‡ Did not sign the Constitution.

Jillson, Dr. B. C., Nashville, Tennessee.
†King, Thomas D., Montreal, Canada.
†Lamborn, Robert H., Philadelphia, Pennsylvania.
†Marissal, F. V., Fall River, Massachusetts.
‡Miller, Judge Samuel [A], New Haven, Connecticut.
‡Morris, D., Ellington, Connecticut.
‡Morgan, DeWitt C., M. D., Baltimore, Maryland.
Pierrepont, H. E. [A], Brooklyn, New York.
Poole, Henry W., Boston, Massachusetts.
*Porter, Prof. John A., New Haven, Connecticut.
*Pugh, Prof. Evan, Centre Co., Pennsylvania.
*Rood, Prof. O. N., Troy, New York.
†Russell, Scott [A], England.
†Silsbee, E. A. [A], Salem, Massachusetts.
†Sims, Dr. J. M. [A], New York, New York.
‡Smith, A. D. [A], Providence, Rhode Island.
Stevenson, Charles L., Charlestown, Massachusetts.
*Tingley, Prof. Joseph, Greencastle, Indiana.
‡Turner, Dr. Henry E. [A], Newport, Rhode Island.
‡Upham, Dr. J. B. [A], Boston, Massachusetts.
*White, Prof. Henry H., Harrodsburg, Kentucky.
‡Wright, A. W., New Haven, Connecticut.

The following signed the Constitution and paid the assessment, but were not elected:—

Bruce, C. E., Ashtabula, Ohio.
Searles, W. H., Cincinnati, Ohio.
Sharswood, William, Philadelphia, Pennsylvania.

The following have voluntarily withdrawn from the Association:—

Astrop, R. F., Burns Co., Virginia.
Boyden, Uriah A., Boston, Massachusetts.
Dyer, David, Albany, New York.
Fairchild, Prof. J. H., Oberlin, Ohio.
Ferland, J. B. A., Quebec, Canada.
Hallowell, Benjamin, Alexandria, Virginia.
Sherwin, Thomas, Dedham, Massachusetts.
White, Aaron, Cazenovia, New York.

LECTURE

ON

THE GULF STREAM.

BY

PROF. A. D. BACHE,

SUPERINTENDENT U. S. COAST SURVEY.

By request of the Association, at their last meeting, at Springfield, I now present a summary of the results of the Gulf Stream explorations made by the officers of the Coast Survey.

The Gulf Stream is the great hydrographic feature of the United States coast, and no survey of the coast could be complete for purposes of navigation, without it. Hence the explorations have been early undertaken and thoroughly carried on. But as it required peculiar means and special adaptation in the officers to this line of research, and did not require a continuous survey, the work has been executed from time to time, as means and officers could be had without interference with the more regular operations of the hydrography.

An act of Congress which refers to this Survey, requires the immediate presentation of its results to Congress, and they have therefore been discussed as soon as procured and have been given to the public.

This is the great sea mark of the coast of the United States, both Gulf and Atlantic, and its qualities as hinderances and aids to navigation require that the navigator should be well informed in regard to it.

In order to present an intelligible summary of the results obtained by the Coast Survey in the short time allowed for a lecture, it is necessary to condense the subject very considerably, to omit matters at all extraneous to the subjects in hand, and to confine myself to a brief and direct statement of *the means employed in examining the stream from its surface to its depths*, *the method of studying the results*, and *of the results themselves.*

The temperatures in and near the Gulf Stream, are among its most striking peculiarities, and therefore have formed one principal object of observation. It will be necessary in order to bring the subject within limits, to confine myself chiefly, at this time, to the consideration of this class of facts and to the results and laws connected with them.

I shall proceed therefore to consider the subject under the following heads:

1. The instruments for determining depths and temperatures and for obtaining specimens of the bottom.

2. The plan of research.

3. The method of discussion of the results.

4. The results, consisting of type-curves of the law of change of temperature with depth, at several characteristic positions. Type-curves showing the distribution of temperatures across the stream, represented by two sets of curves, one in which the variable temperatures at the same depth is shown, and the other in which the variable depth of the same temperature is represented. Upon the diagrams showing these latter curves, the figure of the bottom of the sea is given, where it has been obtained.

Discussion in regard to the cold wall, which is one of the most interesting features of the approach to the Gulf Stream.

5. The limit of accuracy of the results.

6. The figure of the bottom of the ocean below the Gulf Stream.

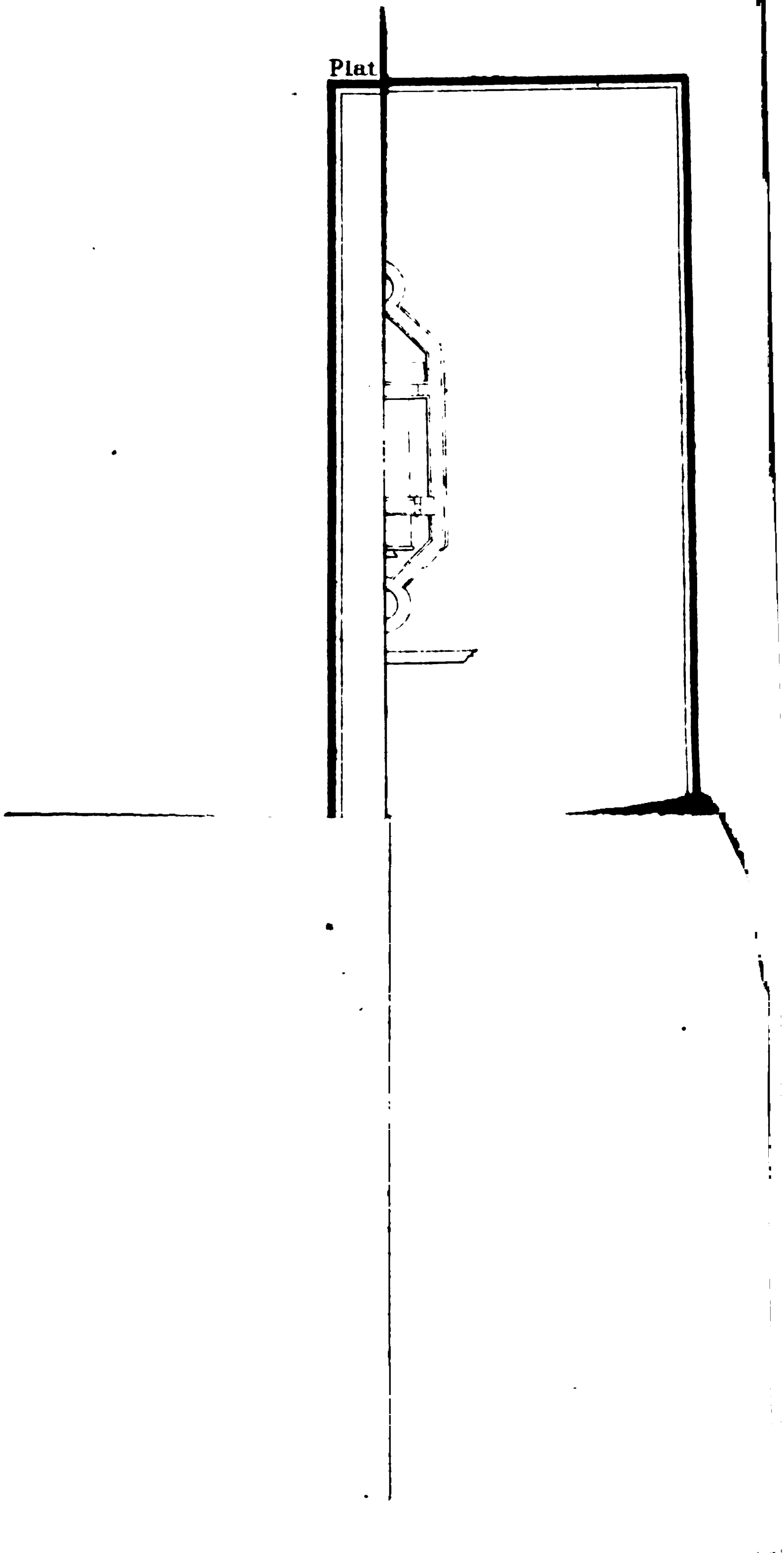
7. The general features of the Gulf Stream as to temperature.

These points are illustrated by diagrams, enabling the eye to follow the results as they are stated.

I. INSTRUMENTS.

1. *For Temperatures.*—The instrument for determining temperatures should fulfil the two conditions of registering its indications and of being unaffected by pressure. The common mercurial thermometer, while it answers perfectly for the determination of temperatures at the surface, fails in both the conditions stated. The ordinary self-registering thermometer, or self-registering metallic thermometer, in the watch form, as made by Breguet, Montandon, and Jürgensen, when provided with a suitable cover to protect it from pressure, answers a good purpose, and has been extensively applied in the course of the observations. As a rule it is only the minimum temperature thermometers that must be used, as the temperatures decrease generally in descending. An ordinary self-registering minimum thermometer placed in a glass globe, was successfully used by Commander Charles H. Davis, and by Lieut. G. M. Bache. It has the disadvantage of taking the temperature slowly, and of being inapplicable below a certain depth. Small hollow cylindrical brass vessels which were divided in two parts closely fitted by grinding, and within which the Breguet thermometers of the watch form were placed, were an improvement upon the glass globe, as taking the temperature of the sea more rapidly, but besides the difficulty of making the joint tight, they were crushed by the pressure, at even moderate depths. The substitution of a globe, for the cylinder, extended the range of these instruments, but the thermometers were often crushed or injured by

the access of sea-water to the interior of the globe. Six's self-registering thermometers, as bearing considerable pressure without injury and without rendering the indications erroneous, and as requiring no case to enclose them, except to prevent breaking from accidental knocks in handling, are very useful. They are still favorites with many of the officers, though others complain of their great liability to derangement, especially if the mercury is not perfectly clean, when the mercurial column easily separates and some skill is required to bring it together. These instruments are from their cheapness still furnished to the parties and are used successfully at depths reaching about one hundred fathoms, and on occasions, considerably lower. Keeping them in order requires the skill of an experimenter, rather than that of an observer, and hence they do not satisfactorily fulfil the conditions of the problem. The metallic thermometer of Joseph Saxton, Esq., of the U. S. Office of Weights and Measures, is a compound coil resembling somewhat the well-known instrument of Breguet. In its construction, two stout ribbons, of silver and platinum — carefully united by silver solder to an intermediate thin plate of gold — are coiled with the more expansible metal in the interior. The gold serves to prevent the tendency of the silver and platinum to separate. The lower end of this coil is fastened to a brass stem passing through the axis of the coil, while its upper end is firmly attached to the base of a short cylinder. The whole motion of the coil as it winds and unwinds with variations of temperature, thus acts to rotate the axial stem. This motion is magnified by multiplying wheels contained in the short cylinder at top, and is registered upon the dial of the instrument by an index, which pushes before it a registering hand, moving with sufficient friction merely to retain its place when thrust forward by the index hand of the thermometer. These instruments are graduated by trial. The brass and silver portions receive a thick coating of gold by the electrotype process, to prevent the action of the sea-water upon them.


Plat

When kept clean by frequent washing in fresh water, and in good order and frequently compared with the standards to guard against accidental derangements, these thermometers answer admirably all the required conditions. The length of the coil measured along its axis should not be less than six inches, as the interposition of wheels to magnify the motion, should, as far as possible, be avoided. The water being all around the coil, which is a good conductor, and has a low specific heat, the instrument readily feels the temperature of the part of the sea where it is exposed, and registers it to less than half a degree (say 0.2) with certainty. The box which covers the coil and indicating part of the thermometer is merely to protect it from accidental injury, and is open so as to permit the sea-water to pass freely through it. Plate IV. gives a view of Saxton's metallic thermometer, and of its various parts in detail. Although there seemed no reason to doubt that this instrument was free from any effects of pressure, it was deemed desirable to actually try it by extreme pressure, and a series of experiments made by J. M. Batchelder, Esq., showed that at pressures less than that corresponding to 600 fathoms, the effect was less than one degree (0°.25 to 1°), and at pressures from 600 to 1,500 fathoms the change amounted to little more than from 7° to 9° Fahr., the index returning when the pressure was removed. For great depths the effects of pressure must be ascertained, as it is specific in each instrument and probably depends chiefly upon some mechanical defect in the construction, perhaps in the soldering.*

The apparatus used in these experiments on the effect of pressure, was a very ingenious one for testing hydraulic engines, by Mr. Thomas Davison of the Novelty Iron Works of New York. Fig. No. 12, Plate IV.

* Gulf Stream Explorations, Third Memoir, Proceedings Amer. Assoc. Adv. Sci., 13th Meeting, Springfield, 1859, and Amer. Jour. Sci. [2] vol. xxix. 1860.

2. *For Depths.*—Where the depth becomes considerable the usual sounding line fails entirely to give it, especially if there is a current, and more especially if there is besides a counter-current. The amount of "stray line" is very variable. This subject has been ably examined of late years by Commanders Maury and S. P. Lee, Lieuts. Berryman, Brooke, and others of our navy, and by Commander Dayman and others of the British navy, and especially by Prof. Trowbridge of the Coast Survey in his memoir read before the Association ("Deep Sea Soundings," by W. P. Trowbridge, Assistant U. S. Coast Survey), at the meeting in Baltimore, and republished in the American Journal of Science and Arts, vol. xxviii. for the year 1858.

The use of Ogden's or Ericcson's leads to one hundred fathoms is still continued by some of the officers of the survey, though, at such depths, nothing better than the common sounding line is in fact required. Massey's lead with Woltman's wheel, as an indicator, has been extensively used of late years. Mr. Saxton's indicator is more simple than Massey's, but acts upon the same principle. To remedy the defect of the turning of the cord of the lead line, two indicators are applied, one on each side of the axis. Prof. Trowbridge's lead modified somewhat from that described at the last meeting of the Association in Baltimore, has recently been tried with good success by Lieut. Comdg. Wilkinson in the last soundings across the straits of Florida for the telegraph to Havana. The most reliable observations heretofore made in the Coast Survey have been with Massey's indicator, the errors are not such as to affect the development of the laws of change at the moderate depth reached in most of the observations, and at great depths the changes are very slow. The new apparatus has the advantage of saving a great deal of time, and therefore inaccuracies from change of position during the sounding are avoided.

3. *For obtaining specimens from the bottom.*—The only satisfactory test of having reached the bottom of the sea at

considerable depths being the bringing up of a specimen, this has been a subject of constant study with us. The different instruments invented by Lieut. Stellwagen, Commander Sands, Lieut. Craven, Lieut. Berryman, Lieut. Brooke, and other officers of our navy, are all in use for different kinds of bottom, and according to the preference given by different hydrographic chiefs. The one most commonly used in these explorations has been Lieut. Stellwagen's invention; a cup placed below the sounding lead, covered by a disk or valve of leather which slides up the stem of the cup and opens when the lead is descending, closing when it is raised. The weight of the lead and the turning of the cord generally suffice to sink the cup into the bottom, filling it, and when the valve is made to close tightly by a piece of flexible leather below the stiff disk, the specimen is not washed out as the lead is drawn up. In Commander Sands's sounding apparatus a spring keeps an outer cylinder over an opening in an inner hollow one, until it reaches the bottom, when the outer cylinder is forced upwards, and the opening at the side of the inner one, which, having a conical termination, penetrates the bottom, and permits a specimen of the bottom to pass in. On raising the lead the spring forces the outer cylinder over the opening, preventing the specimen from being washed out. The only very deep soundings being, as a general rule, in soft bottom, Sands' specimen-cylinder is admirably adapted to that class of work.

II. PLAN OF THE WORK.

The plan of the work was simple. The temperatures were to be ascertained at various depths, at different distances from the coast, on sections as nearly at right angles with the stream as practicable, the sections starting from some point well known in position. The temperatures were to be taken at distances diminishing as the changes of temperature were

more rapid. So in regard to the depths, the observations were to be multiplied in the strata of rapidly varying temperatures near the surface. So in regard to position, when the cold water near the coast was rapidly exchanging for the warm water of the Gulf Stream, the positions diminishing in distance as the source of the warm water was approached.

The vessel's position was determined with reference to some prominent point, Sandy Hook or Cape May, for example, the course run was perpendicular to the supposed axis of the stream, S. E., several positions were taken up in succession and at each the temperatures ascertained at the surface, at 5, 10, 15, 20, 30, 50, 100, 200, 300, 400, 600 fathoms, or depths found to apply more satisfactorily under the general rule, to the position and section. Having crossed the stream, any position found to be desirable could be assumed on returning, and the extreme position reached was verified by the return to the coast.

The summer season was selected for the standard observations, for various reasons, but chiefly for two, namely, that the weather permitted more accurate work, and the phenomena were more likely to be those of equilibrium, when the surface water was more slowly changing its temperature. Our little vessels could not, without considerable danger, be exposed to the roughness of the wind and water in the Gulf Stream in winter, and when we attempted comparative winter observations, disappointment was often the result. The loss of one valuable officer and ten of his crew, and the extreme peril of another in autumnal explorations of the stream, has but too fully justified these precautions. The propriety of selecting the summer for making the observations was completely proved by the success in determining the laws of temperature.

These observations were but incidental to the hydrography of the coast, and hence were prosecuted only when means could be spared from other more pressing and regular parts of the work. It was only a favorable conjuncture with regard to

officers, means, weather, adaptation of vessel, and the like, which gave results even when attempted. Too much credit cannot be assigned to those who have succeeded in this laborious and perilous work, and their names have been kept in close connection with their results, whenever and wherever brought before the public, and they have been carefully preserved in the archives of the survey. Charles H. Davis, George M. Bache, S. P. Lee, Richard Bache, John N. Maffitt, T. A. Craven, Otway H. Berryman, B. F. Sands, and John Wilkinson make up the list of our successful observers in this field within the last sixteen years. Their names you will see attached to the sections run by them, on the general chart of the Gulf Stream presented to you this evening.

The first was run in 1844, from Nantucket south and eastward, by Commander C. H. Davis, now the accomplished Superintendent of the Nautical Almanac, and the last in 1860 by Lieut. John Wilkinson, from the Tortugas, southeast to the coast of Cuba. The work still goes on perseveringly.

The number of sections run has been fourteen, the number of positions on these sections occupied 300, and the number of observations made for temperature 3,600. The limits below which the stream and the adjacent waters have been explored for temperatures are from latitude 23° N., to 41° N., and from longitude 83° W. to 66½° W. from near Havana to near Cape Cod, and from the Tortugas to 9½° E. of Cape Henlopen. The distance along the axis of the Gulf Stream to the most north-eastern point in the North Atlantic, measures nearly 1,400 nautical miles.

III. METHOD OF DISCUSSION OF THE RESULTS.

These have generally been discussed by diagrams, sometimes by analytical formulæ; the former method is generally best adapted to the character and degree of accuracy and circumstances of the observation; the diagrams finally adopted after trials were chiefly of three different kinds, one for the

discussion of the change of temperature with depths, the two others for the change of temperature with position as well as depth. Of the first of these diagrams Nos. 1 and 2, Plate I., are specimens. The depths constitute the ordinates and the temperatures the abscissæ of a curve, showing the law of change of temperature with the depth. Upon the horizontal lines at the top of the paper the temperatures from ten degrees to ten degrees Fahr. are written, and on the vertical line at the side the depths. The separate observations being represented by dots, the curve is drawn with a free hand among them.

The next two classes of diagrams give the distribution of temperatures across the sections. In the first the temperature corresponding to the same depth; in the second the depths corresponding to the same temperatures. In this latter the figure of the bottom is shown when ascertained. In both classes the distances from the cape, or headland, city, or inlet, which is the origin of the section is marked, and the several positions occupied for observing, so that the abscissæ of the curve are the distances from the point of beginning. In the first (see diagram No. 4, Plate I.) the temperatures are marked on the vertical lines at the left side of the diagrams, the ordinates of the curves thus corresponding to temperatures. In the second (see diagram No. 9, Plate I.) the depths are similarly written, the ordinates thus corresponding to depths. The notes or legend, show in the first case to what depths the curves correspond, and in the second to what temperatures. The observations at each position being plotted according to its temperature or depth in the two classes of diagrams, the curve is drawn with a free hand among the points.

It should be observed that the discussion of each season's observations was in general made separately, and that the result of one, two, or three seasons, grouped, were announced separately, leaving to the new observations to confirm, or refute, the conclusions drawn. It is a remarkable fact that with such difficulties in the way, in the character of the phe-

nomena to be observed, in the diversity of seasons and of observers, the phenomena have always been readily deducible from the observations, and that the separate discussions have been confirmations, the following of the preceding; in short that the nature of the medium in which the work has been performed in its relations to heat, has more than compensated for other difficulties, and that the results are more accordant than the elaborate ones obtained from the progress of temperature below the surface of the ground by the experienced and skilful observers who have made them. Few observations have been rejected in the whole series.

I need not notice special diagrams, which will be explained when your attention is called to them.

When the character of the diagrams to be made had been definitely fixed, they were prepared under the direction of the chiefs of the parties, so that I was relieved of the personal labor of representing the results. In the subsequent general discussion I was greatly assisted by Prof. Pendleton, U. S. N., and by Prof. W. P. Trowbridge, Assistant U. S. Coast Survey, who has made a general review of the whole of the results, preparatory to their publication in a volume of the Records and Results of the Coast Survey.

IV. RESULTS.

1. *Type-curves of law of temperature with depths at the most characteristic positions.* — The two most characteristic positions are in the cold current between the land and the Gulf Stream and in the axis of the stream itself.

1. Diagram No. 1, Plate I., is a specimen of the type-curve in the cold current. The long tongue from the surface to about fifty fathoms in depth is the overflow of the warm water of the Gulf Stream, the temperature varying from 81° to about 55°. The temperatures in the mass of water from fifty fathoms down to five hundred fathoms are just such as would take place in a mass of water heated by conduction from the

surface, the law is that of a logarithmic curve, in which the conducting power of sea water is the modulus of the system.

A comparison of many of these curves with the logarithmic form showed that it was applicable to them within the limits of the probable error of the observations. Taking the warm stratum from the Gulf of Mexico above and the cold polar stratum below, the mass of the water between is heated by conduction. The bottom of the sea has not been reached under the axis of the Gulf Stream, north of Cape Lookout on the North Carolina coast.

This form of curve was deduced in 1844 from the observations of Commander Charles H. Davis and was the first discovery made in connection with the then recently commenced systematic exploration of the Gulf Stream by the Coast Survey.

2. Nos. 2, 3, and 3 bis, Plate I., are specimens of the type-curve in the Gulf Stream, taken from the sections off Cape Henry, Cape Hatteras, and Charleston, being characterized by the comparatively short beak or projection, and the persistence of the higher temperature to great depths, as 55° to 425, 450, 550 fathoms, giving the peculiar shape to this curve between fifty and five hundred fathoms.

II. TYPE-CURVES OF DISTRIBUTION OF TEMPERATURE ACROSS THE STREAM.

(*a*) *Curves of temperature at the same depths.*— The sections made are the following, beginning the enumeration at the Gulf of Mexico: 1. Tortugas to Havana. 2. Sombrero Key to Salt Key. 3. Carysfort, L. H., to Cuba. 4. Cape Florida to Bemini. 5. Off Cape Cañaveral. 6. Off St. Augustine. 7. Off St. Simon's, Georgia. 8. Off Charleston. 9. Off Cape Fear. 10. Off Cape Hatteras. 11. Off Cape Henry. 12. Off Cape May. 13. Off Sandy Hook. 14. Off Cape Cod, being on the average one to each hundred miles along the axis of the stream. These are marked on

the general chart, Plate III., the names of the explorers being stated in the column which gives the point of origin of each section.

The Sandy Hook curves, Nos. 4 and 5, Plate I., are among the best of the type-curves of temperature at the same depth, though among the earliest determined. The overflow of the Gulf Stream into the long space occupied by the cold current between it and the shore, mixing in a degree with the cold water, is well shown by the curves *a b* and *c* at the surface, 5 and 10 fathoms, and the still greater admixture with the cold water at 20, 30, and 50 fathoms (*d, e, f*). The whole space from the shore to 240 miles is occupied, however, with comparatively cold water. Then is met the sudden rise to the Gulf Stream shown especially below 50 fathoms, and termed so appropriately by Lieut. George M. Bache the "cold wall," that navigators have not hesitated to receive the term into use; next the hot water of the Gulf Stream, rising to a maximum of 82°, then falling to a minimum of 80°, rising to a second maximum of 81½°, falling to a second minimum of 78°, and rising from this toward a third maximum. With these results the curves at 5 and 10 fathoms, and those at 20, 30, 50, 70, 100, and 150 fathoms agree, and, with characteristic differences, those of 200, 300, 400, and 500 fathoms.

The cold wall at 20 fathoms shows a rise of 19° in 25 miles, three quarters of a degree to a mile, and at 200 fathoms of 16°, in the same distance; at the surface it is nearly 8° in 50 miles. The cold water between the Gulf Stream and the shore has two well-marked maxima and two minima in it, of which one seems to correspond in position to the sudden deepening of the water 100 miles from Sandy Hook, as shown by the Coast Survey off-shore chart between Gay Head and Cape Henlopen.

These results are more distinctly seen by grouping the curves into natural groups and taking the mean of their indications. Diagram No. 5, Plate I., gives the group of six curves from the surface to 30 fathoms, of four curves from 40

to 100 fathoms, both inclusive of 200, 300, and the single curve at 400.

Similar groups are shown on Diagram No. 6, Plate I, from Cape Henry, the cold wall, three maxima of temperature and three minima being very distinctly seen. The results of three different explorations of this section, by three different officers, in three different years, are shown upon the same diagram. The coincidence of result could hardly be better. The average of the whole of the observations is shown in No. 6 bis, Plate I.

The cold wall here gives a change of 22½° in 50 miles from the curves between 0 and 30 fathoms, and 18° in 50 miles in the mean of 200, 300, and 400 fathoms.

The average of the three years comes out beautifully on Diagram No. 6 bis, Plate I. The Charleston curves are shown upon No. 7, Plate I. They are less regular than those just given, for reasons which will appear when I come to speak of the second class of diagrams.

The conclusions deduced from the examination of all the sections between Cape Florida and Sandy Hook is, that the Gulf Stream is divided into alternate bands of hot, or warm, and cool or cold water, the most distinct of which is that containing the axis of the Gulf Stream.

That between the stream and the coast there is a fall of temperature so sudden that it has been aptly called the cold wall, less distinct at the surface and where the overflow from the Gulf Stream passes furthest toward the shore, but still distinctly marked even at the surface.

Navigators have noticed these changes of temperature and have supposed themselves at each occurrence of warmer water to be in the hottest water of the stream, and so have been greatly embarrassed and have deemed the phenomena and limits of the Gulf Stream to be very irregular.

The cold water between the Gulf Stream and the shore has also bands less regular than those beyond the axis of warmer and cooler water.

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The intrusive cool water in the Gulf Stream on the Sandy Hook section was distinctly recognized in 1846 by Lieut. Geo. M. Bache, who, from the facts observed, supposed it to represent a division of the warm water of the stream into two branches.

Passing through the Straits of Florida between the keys and reefs and the coast of Cuba we have, after going beyond Cape Florida, a different type-curve. The cold wall is less distinctly marked, and the rise of temperature is less marked. It rises however to an axis near the coast of Cuba. Throughout the length of the strait there is but one maximum of temperature, and the bands belonging to the Atlantic regimen do not occur in the straits. (See diagrams Nos. 3, 4, 5, 6, Plate II.) The cause of this change of regimen will be seen in presenting the other form of diagram.

(*b*) *Curves of depths at the same temperature.* — I have selected curves from the southern portions of the work, partly because the bottom has been struck in the sections, and the diagrams show its sections as well as those of the stream, and partly to show how fully the deductions in regard to the divisions of the stream apply to these, as well as the more northern sections. The Charleston section of Lieut. Maffitt is given on diagram No. 9, Plate I. The surface curve, notwithstanding the disturbance by a storm, shows the cold wall (see also No. 7), the axis, and two other maxima, the corresponding minima, a maximum within the cold current which is not therefore, as has been supposed, cut off at Hatteras, the curve of 72° reaching to the coast, and 77° nearly reaching it. The Cape Florida diagrams (Nos. 3 and 7, Plate II.) give two maxima with indications of a third, and the corresponding minima. The cold wall cannot be recognized upon it, probably for the want of one or two more positions.

The form of the bottom delineated on these two sections, namely the Charleston and Cape Florida sections, is remarkable, and applies to the sections between them as far as explored. First is a gentle slope, then a sudden descent, a second

steep pitch to a considerable depth, a range of hills, a valley, and a second range.

The correspondence of these features with the bands of temperature is plainly marked. The cold water lies in the valleys, and passing along the bottom rises upon the tops of the hills. The discovery of this range of hills was made at nearly the same time by Lieut. Maffitt on the Charleston section, and by Lieut. Craven on the St. Simon's section. Diagram No. 9, Plate I., shows this connection in a very striking manner, as does also No. 7, Plate II., and the figure of the bottom of the Straits of Florida, shows why there are no bands formed prior to passing Cape Florida, in other words, why the regimen of the stream is different in the straits and in the Atlantic. In the straits we see (No. 9, Plate II.) that, after leaving the United States shore and the comparatively flat surface extending to the reefs, there is a rapid descent toward the Cuban side of the strait, the axis of the Gulf Stream being found in the deep hollow of that side of the strait.

These results, with a more elaborate discussion of them, were presented at the last meeting of the Association. It would seem, from the configuration of the bottom, that the cold stream at the bottom of the Straits of Florida divides, one portion passing to the north and west into the Gulf of Mexico, and the other around the western end of the Island of Cuba. That the polar stream still occupies the bottom of the strait is shown by temperature of 35° Fahr. being reached at 600 fathoms from the surface off Havana.

Do these bands correspond throughout their length to the form of the bottom of the sea? This is not yet made out, many as have been the attempts to reach the considerable depths off the more northern sections. Three officers have attempted to sound out the Cape Cod section, but the cold wall is all that has been reached thus far. The range of hills nearest to the coast, has been traced from the coast of Georgia, by Commander Sands, to off Cape Lookout.

III. THE COLD WALL.

The cold wall extends, with varying dimensions and changes of its peculiar features, all along the coast where the stream has been examined. A diagram showing the features of the cold wall on the various Atlantic sections and those of the Straits of Florida is given in No. 10, Plate I. Table No. 1 shows the distance of the cold wall from the coast, and the dimensions of the Atlantic bands of the Gulf Stream.

The table shows that at Cape Florida and Cape Hatteras the cold wall is nearest to the coast. The distance of the axis of the stream from the coast will be found by adding half the numbers in the second column to those in the first column. It is obvious from these numbers, when taken in connection with the longitudes of the points where the sections originate, that the earth's motion is not the sole determining cause of the direction of the axis of the stream, a result which a more elaborate investigation of the movements from parallel to parallel confirms. In the portions of its course between Cape Florida and Mosquito Inlet (3½° of latitude) the curve is actually slightly to the westward.

TABLE I.—DISTANCE OF THE COLD WALL FROM THE SHORE, AND WIDTHS OF THE SEVERAL BANDS OF COLD AND WARM WATER IN THE GULF STREAM, MEASURED ON THE LINES OF THE SECTIONS.

Names of Sections.	Distance of cold wall from shore, in miles.	Width of 1st maximum or warm band.	Width of 2d minimum or cold band.	Width of 2d maximum.	Width of Gulf Stream proper.	Width of 3d minimum or cold band.	Width of 3d maximum or warm band.	Width of 4th minimum or cold band.
Sandy Hook,	240	60	30	37	127	60	50	Indef.
Cape May,	125	55	30	40	125	70	65	70
Cape Henry,	95	45	32	47	124	80	60	50
Cape Hatteras,	30	47	25	45	117	37	75	70
Cape Fear,	60	30	20	37	87	30	60	25
Charleston,	62	25	15	30	67	26	35	—
St. Simons,	87	25	13	20	58	25	25	—
St. Augustine,	70	20	13	12	47	22	20	—
Cape Cañaveral,	35	20	—	—	35	14	12	—
Cape Florida,	10	25	—	—	25	5	—	—

NOTE.—The widths of the bands beyond the 2d maximum, and north of Cape Hatteras, are somewhat indefinite.

The table shows a width in the Gulf Stream proper along the Atlantic coast of from 25 miles off Cape Florida to 127 miles off Sandy Hook. The warm water at say fifteen fathoms, is from 30 to 150 miles in width. The stream widens each way from Cape Florida. These several divisions of the Atlantic stream lose a portion of their distinctness as we pass northward and eastward, the stream widening.

V. LIMIT OF ACCURACY OF THE DETERMINATIONS.

There are two modes by which the limits of accuracy of these results may be tested, by one of which their permanency is also tried. In this latter mode the sections are run over in different years, or in the same year by different officers, so as to connect the observations of one year with those of the next, or of one officer with that of another. Table No. 2 shows that the relative results are reproduced from year to year with less variability than those of the mean temperature of the section; and hence the permanency of the bands and the possibility of observing them with the requisite precision must be admitted. On the Cape Henry section, which was explored three times, the positions of the cold wall and of the axis of the stream were reproduced within 5½ miles, and those of the succeeding points of maximum and minimum temperatures within 7½ miles. As the positions at sea are liable to an uncertainty of some three to five miles, it must be admitted that the permanency of the bands and the accuracy of the observations of them are fully proved.

The Cape Henry section was run over by Lieuts. G. M. Bache, S. P. Lee, and Richard Bache, the Hatteras section by Lieuts. Richard Bache and J. N. Maffitt, and the Charleston section by Lieuts. J. N. Maffitt and T. A. Craven.

TABLE II. — TABLE SHOWING THE PROBABLE UNCERTAINTY IN THE DETERMINATION OF MAXIMUM AND MINIMUM POINTS, BY RUNNING THE SAME SECTION OVER IN DIFFERENT YEARS BY DIFFERENT OBSERVERS.

Cape Henry Section.							
Dates and names of observers.	Mean distances from the shore, in miles, from the curves representing the groups.						
	Cold wall or 1st min.	Axis of 1st max.	Second min.	Second max.	Third min.	Third max.	Fourth min.
Lieut. G. M. Bache, 1846,	93	135	187	218	260	320	369
" S. P. Lee, 1847,	91	146	185	215	291	337	338
" R. Bache, 1848,	97	146	180	197	287	328	370
Means for three years,	84	142	184	210	279	328	370
Probable error for each year,	5.85	4.27	2.42	7.62	11.31	5.71	7.18
Cape Hatteras Section.							
Lieut. R. Bache, 1848,	—	90	134	162	214	286	355
" J. N. Maffitt, 1853,	—	75	125	157	211	256	322
Means for two years,	—	82	129	159	212	266	338
Probable error for each year,	—	6.4	4.3	2.4	1.5	15	16
Means for both sections.	5.85	5.3	3.4	5.0	6.4	1.04	1.16

Average uncertainty of maxima and minima, ... 6.9 miles.
" " " cold wall and axis, ... 5.5 "
" " " all the other points, ... 7.4 "

The other mode of testing the results is by the comparison of the remarkable points in the different sections, each one belonging to a different position and therefore being entirely independent of the other in its determination. It is established as a general law, that the cold wall and axis of the hottest water change their position from the surface to the depth of six hundred fathoms slowly and by an ascertained progression, and that the succeeding maximum and minimum points are at the same distance from the shore, nearly, at different depths, or in a vertical line at all the different depths. The positions of these points as shown by the observations at different depths, become thus the test of the permanency of their positions, and of the accuracy with which they have been ascertained. Table 3 gives the probable error of the mean of the determinations of each point, including the cold wall minimum, the axis maximum, and the successive minima and maxima to the fourth minimum inclusive. These results show that the cold wall minimum is ascertained, on the average, within 0.83 mile, the axis maximum within two miles and a half, the second minimum within two miles and a half, the second maximum and third minimum and third maximum within four miles, and the fourth maximum within eight and a half miles, all being satisfactory except the last, which of course is in reality loosely defined. The Hatteras result for the axis of the stream makes the probable error considerably larger than it would otherwise be, probably from the fact that the proximity of the bottom of the sea makes the result less permanent than in the other cases. Without this result, the mean probable error would be 1.1 mile.

TABLE III.—RECAPITULATION, SHOWING THE VALUE OF THE PROBABLE ERROR OF DETERMINATION OF THE BANDS FOR EACH SECTION AND THE AVERAGE OF THE WHOLE.

Sections.	Probable errors.						
	1st min.	1st max.	2d min.	2d max.	3d min.	3d max.	4th min.
Sandy Hook,		.75		3.94	7.99		
Cape May,	.82	1.25	2.54	1.57		4.03	4.87
Cape Henry, 3 years,	.84	.61	.55	1.70	1.06	.94	3.42
Cape Hatteras, 2 years,		6.77	6.36	9.31	5 69	6.23	
Cape Fear,		1.25			2.98	3.49	13.37
Charleston,	1.25	1.57	.72	2.09	2.40	.82	
St. Simons,		.74	1.27	.41			
St. Augustine,	.52	.51	.44	.44	.55		
Cape Cañaveral,	.95	1.69	.39				
Mean probable error,	.83	2.49	2.49	4.00	4.01	3.71	8.45

While these results are so permanent, the mean temperatures of the sections change considerably from year to year. The average temperature between the surface and 400 fathoms beyond, or outside of the cold wall on the Sandy Hook section, in 1846, was as high as that on the Cape Henry section in 1848, and that on the Cape Fear section in 1853 within a degree of that of the St. Augustine section in 1853, while the Cape Hatteras section in 1848 and in 1853 differed two degrees in mean temperatures. Again, the temperatures from the surface to thirty fathoms, just below the axis of the stream, in the Sandy Hook section, in August, 1846, was either as high or higher than those on the Cañaveral section in June, 1853. In general, the Cape May section in 1846 and the mean of the Cape Henry section of 1846, 1847, and 1848 are warmer at the same depths than the sections south of it were in 1848 and 1853.

These results show that there are great changes in temperature from year to year, and probably from season to season. Some progress has been made in connecting these results in a general way with the changes of weather in the Gulf of Mexico.

The depths at which the results are easily determined, and where they are characteristic and as permanent as the phenomena permit, is thirty fathoms.

VI. FIGURE OF THE BOTTOM OF THE SEA, BELOW THE GULF STREAM.

We have seen that in cross sections there is a great resemblance in the bottom of the sea off our coast to the region of land more removed from the coast-line in the interior. The top of the first range of hills (see Diagram No. 9, Plate I.) is 1,500 feet above the valley to the eastward of it, distant twelve miles; and the top of the second range 600 feet above the same valley, distant fifteen miles. The first slope is 125 feet, and the second is forty feet, to the mile. The bottom of the sea, from the Tortugas section to that of Cape Florida, rises from 800 to 325 fathoms, and from the same point descends, in passing northward and eastward. The Cape Florida section showed that there then was present a ridge of comparatively cold water, since the division into bands should apply along the stream as well as in the direction of its cross sections. The temperature of 40° is in fact reached on that section at 300 fathoms, and, as well as can be judged from the results in the separate sections, there are divisions of this sort. The diagram No. 2, Plate II., shows where the curves of 50° and 45° are found upon the different sections, and indicates a rise on the Charleston section and a sharp descent from Charleston to Cape Fear.

VII. GENERAL FEATURES OF THE GULF STREAM.

pon the general diagram now presented to the members te III.), the general features of the Gulf Stream are repre-ed from the Tortugas to the Cape Cod section. Passing g the Cuban coast, the temperature in June was found to bout 84°, or 8° above the mean temperature of Key West, iven by the Surgeon-General's report. The current here eble, but sufficient to cause it to be sought by sailing els making to windward, and even by steamers. Issuing the Straits of Bemini, the stream is turned northward e land, which confines and directs its course. Its effective ity is not derived from difference of temperature, as the rvations abundantly show, the greatest relative differ-s being in fact crosswise of the stream. The direction is a little west of north, and the velocity is from three to miles per hour. The temperature bands now begin. The om of the sea, which was one slope and counter slope ss the Florida Straits, is here corrugated; the depth, ad of being unfathomable, as has heretofore been sup-d, is but 325 fathoms, in which depth the two currents, the poles near the bottom, and from the Gulf at the top, t pass each other. While the surface water is above 80°, near the bottom is as low as 40°.

he stream just north of Mosquito Inlet begins to bend to eastward of north, and off St. Augustine has a decided o the eastward. While flowing thus onward, the warm r seeks the sides of the channel overflowing towards the t of Florida, and towards the Bahamas, but not as rapidly t moves on north. Between St. Augustine and Cape eras the set of the stream and the trend of the coast r but little, making five degrees of easting in five degrees orthing. At Hatteras it curves to the northward, and runs easterly, making about three degrees of northing in e degrees of easting. In the latitude of Cape Charles it

turns quite to the eastward, having a velocity of between one and one mile and a half the hour.

That this curve follows the general sweep of the coast under water, appears most probable; the coast line, the curve of one hundred fathoms, and the ranges of hills discovered by Lieuts. Maffitt and Craven, all seem to indicate it. That the direction of the stream is given in a general way by the configuration of the bottom of the sea, is hardly possible to doubt, while admitting that it receives modification from other, and perhaps more general, causes. The after progress of this mighty stream and of its branches, if it does divide, remains yet to be traced, and so also its heading in the Gulf of Mexico.

I forbear to mingle doubtful speculation upon causes, with the inductions in regard to temperatures, which it has been the object of these observations to supply, and of this lecture to bring to your notice.

PROCEEDINGS

OF THE

NEWPORT MEETING, 1860.

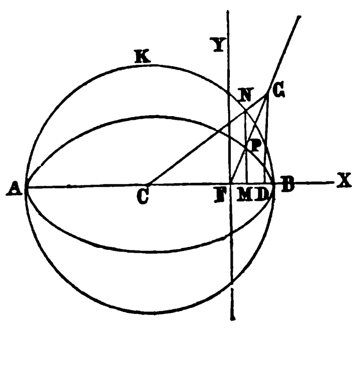
COMMUNICATIONS.

A. MATHEMATICS, PHYSICS, AND CHEMISTRY.

I. MATHEMATICS AND PHYSICS.

1. On the Possibility of expressing the Polar Coördinates of the Asteroids by converging Series, admitting of Tabulation. By Prof. George W. Coaklay, of Hagerstown, Maryland.

Let APB be the elliptical orbit of any planet, AKB the circle described upon the major-axis as a diameter, F the focus occupied by the sun's centre, $FP = r$ the planet's radius-vector at any moment.



Hence $PFM = v =$ the true anomaly, $NCM = E =$ the eccentric anomaly, $CN = a =$ the mean distance. Let $FM = x$; then $x = r \cos v$, $CM = ae + x = a \cos E$, $e =$ the eccentricity.

Hence $$r \cos v = a(\cos E - e). \qquad (1)$$

Produce FP and CN to meet in G, and draw GD perpendicular to CX; let $FG = r_1$ $FD = x_1$ $CG = m$; then

$$x_1 = r_1 \cos v,\ ae + x_1 = m \cos E,\ \therefore m \cos E = ae + r_1 \cos v. \qquad (2)$$

Also $$\frac{m}{r_1} = \frac{\sin v}{\sin E},\ \text{or}\ m = r_1 \frac{\sin v}{\sin E}. \qquad (3)$$

But the polar equation of the orbit APB is $r = \dfrac{a(1-e^2)}{1+e\cos v}$. (4)

Dividing (1) by (4) we have $\cos v = \dfrac{(1+e\cos v)(\cos E - e)}{1-e^2}$. (5)

Hence $\cos v - \underline{e^2 \cos v} = \cos E - e + e \cos v \cos E - \underline{e^2 \cos v} \therefore$

$$\therefore \cos E = \frac{e + \cos v}{1 + e\cos v}. \qquad (6)$$

$$\therefore \sin E = \frac{\sqrt{1-e^2}\sin v}{1 + e\cos v}; \qquad (7)$$

where it is evident that $\sqrt{1-e^2}$ must be taken *positively*.

From (2), (3) and (6) we have

$$r_1 \frac{\sin v}{\sin E} \frac{e + \cos v}{1 + e \cos v} = ae + r_1 \cos v. \qquad (8)$$

Eliminating $\sin E$ by (7), we have

$$\frac{r_1(e + \cos v)}{\sqrt{1-e^2}} = ae + r_1 \cos v.$$

Hence,

$$r_1 \cdot [e + (1 - \sqrt{1-e^2}) \cdot \cos v] = ae\sqrt{1-e^2}$$

$$r_1 = \frac{a\sqrt{1-e^2}}{1 + \dfrac{1-\sqrt{1-e^2}}{e} \cdot \cos v}. \qquad (9)$$

Let

$$e_1 = \frac{1-\sqrt{1-e^2}}{e} = \frac{e}{1+\sqrt{1-e^2}}. \qquad (10)$$

And,

$$a \sqrt{1 - e^2} = a_1 (1 - e_1^2). \tag{11}$$

Hence,

$$r_1 = \frac{a_1 (1 - e_1^2)}{1 + e_1 \cos v}. \tag{12}$$

It is therefore evident that the locus of the point G is an ellipse, the radius-vector $F\,G$ coinciding with that of the planet *in direction.* I shall call this ellipse the *first derived orbit* of the planet.

According to the usual notation, let

$$e = \sin \varphi\text{, and } e_1 = \sin \varphi_1;$$

Then

$$\sin \varphi_1 = \frac{\sin \varphi}{1 + \cos \varphi} = \frac{2 \sin \frac{1}{2} \varphi \cos \frac{1}{2} \varphi}{2 \cos^2 \frac{1}{2} \varphi} = \tan \tfrac{1}{2} \varphi.$$

Or,

$$\sin \varphi_1 = \tan \tfrac{1}{2} \varphi. \tag{13}$$

It is also easy to prove that

$$e = 2 . \frac{e_1}{1 + e_1^2}. \tag{14}$$

Whence,

$$e^n = 2^n . e_1^n . (1 + e_1^2)^{-n}. \tag{15}$$

It is well known that the radius vector r, and the true anomaly v, of any planet can be developed in series arranged according to the ascending powers of the eccentricity e; hence by substitution from (15) and developing, those coördinates may in like manner be expressed in series arranged according to the powers of the much smaller eccentricity e_1; which I apprehend will be found advantageous in the case of the asteroids. If, however, it should happen that e_1 is still too great, then a *second derived orbit* may be obtained from the first, exactly as the first was derived from the planet's true orbit, and in which we should have the relations,

$$\sin \varphi_2 = \tan \tfrac{1}{2} \varphi_1,\ e_1^n = 2^n . e_2^n . (1 + e_2^2)^{-n},$$

by the substitution of which the planet's coördinates would be expressed in terms of the powers of e_2. This process may evidently be carried on to any required extent; and even the perturbations of the asteroids, which depend upon the eccentricity, may be expressed by a similar transformation, with more converging series than in the usual manner. Hence they may admit of tabulation like the older and less eccentric planets.

2. Theory connected with the Solar Spots (Abstract). By Prof. C. W. Hackley of New York.

Observations made by me for a number of months, indicate that when the penumbræ of the solar spots are equally broad on the east and west side of the nuclei, the temperature at the earth rises much and rapidly within a day or two after, and suddenly falls again below its normal state, the fall being often accompanied by an aurora.

I have been disposed to attribute this to an emanation of transparent elastic matter, from the opening in the pholosphere of the sun, which constitutes the macula. This matter reaching the vicinity of the earth's attraction with great velocity, contracts by the action of the attraction, giving off heat, and expands again on passing away from the earth, producing cold; and affording the material for the Aurora Borealis, especially when coming in contact with the terrestrial atmosphere.

The force requisite to produce such a velocity must be enormous, but not greater than that to which the comets have been subjected, in their origin; — and I am inclined to attribute this origin to the maculæ of either our own sun or some other, the matter of the emanation condensing sufficiently by the radiation of its own heat to become visible by reflection of the solar light when the comet is seen.

The corona in total eclipses of the sun, I suppose to be the same emanations of elastic matter through the intervals of the luculi. The diameter of the corona is too great for an atmosphere, and its effects on certain comets as that in 1843, should have been greater if of an atmospheric nature.

The sun I take to be the source not only of meteors which may be the emanations I have supposed, after contact with the atmosphere, but also of aerolites and showers of cinders. The sun has an enormous activity for the production of these bodies, whilst the moon, to which they have been attributed, however great may have evidently been its activity, shows on the surface towards us at least that this is now extinct and dead.

Finally, it has occurred to me that this emanation of transparent matter from our own and other suns, might be the substance of the

ether ejected with sufficient velocity to reach other systems as do the comets, and filling the intermediate spaces between system and system, affording a medium capable of transmitting the vibrations produced by the rapid action of the pholosphere and thus, the sensation of light.

In confirmation of these views, in addition to the observations alluded to above, I may mention the following: —

1. Professor Olmstead in a treatise on the aurora, published by the Smithsonian Institution in 1856, makes its secular period about sixty-five years, nearly a multiple of that of the solar spots 11.111 years. A careful examination of his history of the aurora shows a period corresponding with that of the spots. He also notices the connection between the aurora and meteors.

2. The period of an unexplained inequality in the solar perigee noticed by Mr. Leverrier, Comptes Rendus, Tome xxxvi. p. 351, is 66⅔ years, an exact multiple of the period of the solar spots.

3. A diminution of the mean motion of the planet Mercury, on comparison of the observations of the last forty years with one another, and with ancient observations (noticed by M. Leverrier in the "annals of the Observatory of Paris," Vol. 1, p. 38), without any corresponding diminution in the mean distance, may be explained by a combination of a resisting medium with an outward movement of the same.

4. The daily published records of temperature throughout Europe and Algiers, and those of America from Nova Scotia to San Francisco, show that the maxima of temperature follow upon the axis of the opening of a solar spot being directed towards the earth, and that this is universal and simultaneous, or nearly so over the whole earth.

The account given by Mr. Dawes, in the Ast. Nach. of a remarkable spot seen by him in October, 1859, shows that the force producing the opening in the pholosphere is from within outward, since he saw the facula raised above the edge of the disc, like an elevated mountain ridge on the moon. The opening of this spot was very oblique. The spots seem most frequently to have the axis of the opening directed towards the earth, when on the east of the centre of the sun.

In No. 1207 of the Astronomesche Nachrichten, Secchi notices extraordinary heat in July and August, 1859, without a proper state of the wind to produce it. He says: Si cette temperature eleveè est generale il faudra en chercher la cause allieurs que dans les vents et dans l'atmosphere terrestre peutètre le soleil lui-mene et plus puissant cette

annè ici." The same remarkable temperature was exhibited on this side the Atlantic both north and south of the equator at the same time.

Alluding again to this same period in another paper, in connection with a day aurora, Secchi says, during these extraordinary heats, a very large solar spot was visible without instruments. And yet Secchi himself, shows by experiments with the Melloni apparatus, that the spots are not so hot as the luminous portions of the sun. Secchi's experiments also prove the equatorial portions of the sun to be hotter than any other. Does not this disprove Herschel's theory of the formation of the spots rather than confirm it? If the heat in the equatorial regions be prevented from radiating there by the greater thickness of the atmosphere, ought not this portion of the sun to show a lower temperature by the Melloni apparatus?

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3. ON THE ECLIPSE OF THE SUN OF THE 18th JULY, 1860, from Observations taken at St. Martin, Isle Jesus, Canada East,—Lat. 45° 32′ North; Longitude 73° 36′ West, 118 feet above the level of the Sea. By Prof. CHARLES SMALLWOOD, St. Martin, Isle Jesus, Canada East.

As seen by the geographical coördinates of the place, the obscuration was only partial, and south of the line of the total eclipse — the first constant occurred at 7 hours 10 minutes in the morning (St. Martin's mean time), at a little west of the sun's northern limb (*direct view*), the sky at 6 A.M. was somewhat clouded by "*cumulus*" clouds, which cleared away and left the first contact visible, a few "*cumuli*" passed over the sun at 7 hours 30 minutes, and a few "*strati*" were seen in the north-east near the horizon — the line of contact was sharp and well defined, the inferior cusp at one time seemed somewhat "straightened," or as it were elongated backwards, but sharp. The sun's disc presented several spots, one of a large size, which had been visible for some days, — the moon's shadow passed directly over this, and a smaller spot; there was no apparent "*bluntness*" at the cusp, nor were the edges "*jagged*" or "*serrated*" at any time; the surface of the moon appeared very dark in contrast with the bright and silvery disc of the sun; a "*haziness*" was observed on the outer edge of the sun; the last contact, which was very distinct, was south of the sun's eastern limb.

The usual 6 o'clock morning observation is thus recorded: — Barometer 29.826 inches, temperature of the air 62° 3′, minimum temperature during the night 58° 6′, lowest point of terrestrial radiation 47° 8′, wind S. S. W., calm, cirr. cum. clouds $\frac{8}{10}$, ozoneometer indicated $\frac{2}{10}$, Volta's electrometer marked 1°: negative intensity of the sun's rays 63° 4′. Sun somewhat clouded. Aurora Borealis visible during the night.

TABLE SHOWING THE HYGROMATRICAL STATE OF THE ATMOSPHERE.

St. Martin's mean time.	Gaseous pressure of the Atmosphere.	Dew point.	Elastic force of vapor.	Weight of vapor in a cubic foot of air.	Degree of humidity in reduction.	Remarks.
h. m.	Inches.	Degrees.	Inches.	Grains.	1,000.	
7.00	29.409	55°.2	0.445	4.99	0.798	Cloudy.
7.10	.416	55°.8	0.425	4.72	0.711	1st contact.
7.15	.416	55°.8	0.425	4.72	0.687	
7.30	.436	52°.2	0.402	4.47	0.651	Cumuli.
7.40	.428	53°.8	0.425	4.72	0.687	Light cirri.
8.00	.447	54°.1	0.430	4.67	0.652	
8.10	.405	56°.4	0.464	5.14	0.726	
8.13	.384	57°.2	0.476	5.51	0.798	Greatest obscuration.
8.30	.362	56°.4	0.464	5.14	0.726	
8.45	.340	58°.2	0.494	5.39	0.731	
9.00	.398	56°.2	0.461	5.09	0.657	
9.15	.353	58°.0	0.489	5.38	0.672	
9.20	.344	58°.9	0.501	5.46	0.620	Last contact.

This table shows the increase of vapor and amount of humidity during the eclipse, its gradual increase up to the moment of greatest obscuration is well shown.

TABLE SHOWING THE ATMOSPHERIC PRESSURE, TEMPERATURE, AND RADIATION.

St. Martin's mean time.	Barometer corrected and reduced to 32 F.	Temperature of the air F.	Intensity of the sun's rays.	Intensity of terrestrial radiation.	Remarks.
h.m.	Inches.	Degrees.	Degrees.	Degrees.	
7.00	29.854	62°.7	81° 3	54°.0	Zenith clear.
.10	.841	64°.7	90°.3		1st contact.
.15	.841	65°.0	87°.8		
.30	.838	65°.3	87°.1	52°.7	Cumuli.
.40	.853	65°.0	86°.0	52°.5	Light cirri.
8.00	.877	66°.0	84°.2		
.10	.869	66°.0	78°.4		
.13	.860	63°.9	77°.4	57°.0	Greatest obscuration.
.30	.826	66°.2	78°.0	57°.0	
.45	.854	67°.0	82°.4	60°.0	
9.00	.859	69°.0	86°.4	62°.5	
.15	.842	70°.3	96°.2		
.20	.845	71°.1	95°.2		Last contact.

This table shows the decrease of temperature of the air, and also the decrease in

the intensity of the solar rays; the lowest reading of the barometer occurred seventeen minutes after the greatest obscuration, the lowest temperature, as might be expected, occurred at the moment of greatest obscuration.

Polarimeter. Nothing unusual could be detected either in the intensity of the sky polarization or in the normal angle, compared with observations made some days previous. The zenith was quite free from clouds during the whole period of the eclipse.

Solar Spectrum. The solar spectrum was carefully observed during the whole time, but nothing was seen either in extent or definition from its usual appearance. An apparatus erected for the purpose of examining Fraunhofer's black lines in the solar spectrum was used both with direct and reflected light, and indicated nothing but a slight faintness or nebulous appearance in the red end of the solar spectrum; the lines were distinct, and were well shown in the other colored rays. An upright staff showed no "*flickering*" or "*wavering*" of its shadow.

Photometric Scale. Sensitive paper prepared and exposed successively for a given period of time showed very positive and interesting results, the shades varying considerably during the increase and decrease of the partial covering of the solar disc. Chromotype paper also furnished similar results from given periods of exposure. Our clever photographic artist, Mr. Notman of Montreal, found similar results in the use of his chemicals. I am indebted to him for nine photographs obtained by him during the eclipse:

The ozoneometer exhibited no sensible difference.

Atmospheric Electricity. The electrometers at 10 P. M. on the 17th day, indicated only 1° of a negative character, and at 7 A. M. of the 18th day remained the same both in degree and kind; there was a slight increase in intensity from 8 till 8.48 A. M. of 1½°, but possessing the same negative or resinous character.

The only effects on the animal creation which were observed was the frequent crowing of cocks; and on the vegetable kingdom the flowers of the "*morning glories.*" *Convolvulus* seemed somewhat drooping.

The photographic artist (Mr. Notman), to whom I have referred, obtained some beautiful ambrotypes of the eclipse, nine in number, by direct view, and is a fine specimen of instantaneous photography.

4. ABSTRACT OF THE PRINCIPAL RESULTS OF THE ASTRONOMICAL OBSERVATIONS AT VAN RENSSELAER HARBOR AND OTHER PLACES NEAR THE NORTH-WEST COAST OF GREENLAND, MADE BY THE SECOND GRINNELL EXPEDITION, UNDER COMMAND OF Dr. E. K. KANE, U. S. N., during 1853, 1854, 1855 (from a reduction and discussion by CHARLES A. SCHOTT), Assistant Coast Survey.* Presented by Prof. A. D. BACHE.

OF the astronomical observations made by Dr. Kane's party, those for the longitude of Van Rensselaer Harbor, the winter-quarters of the expedition, were most numerous and carefully attended to. The geographical location of the shore line, traced by the expedition, depends for its longitude on that of Van Rensselaer harbor as the central meridian. The latitude of Van Rensselaer harbor was likewise carefully determined, as far as the instrumental means of the expedition permitted. The astronomical and geodetic material collected by the various travelling parties, and required for the geographical position of their tracks, is given in Appendix No. 6 to the second volume of the Narrative. Part of this material was collated with the manuscript, and the revised results will be given in this paper after the result of the discussion of the latitude and longitude of the winter-quarters. The record of the observations discussed is taken from the original logbook or other manuscript documents, belonging to the expedition. The astronomical observations were under the special care of Mr. Augustus Sontag. The principal instruments for the determination of the geographical positions were sextants, a Gambey theodolite, a transit instrument, and five mean time chronometers.

Fern Rock observatory was established on the northernmost of the rocky group of islets in Van Rensselaer Harbor, of which a general survey was made on August 25, 1853. The observatory consisted of four walls of granite blocks, cemented together with moss and water, and the aid of frost. These walls were covered with a substantial wooden roof, with openings in the direction of the meridian and prime

* For the complete paper, see Smithsonian Contributions to Knowledge, Vol. XII

vertical. The transit and theodolite were mounted on piers, formed by a conglomerate of gravel and ice, well rammed down, in iron-hooped pemmican casks and cemented by freezing water. These piers were found as firm as the rock on which they rested.

Observations for latitude of Van Rensselaer Harbor. The first observation for latitude was made on Sept. 12, 1853, with the theodolite. Later observations were obtained by means of a sextant and artificial horizon. The Gambey theodolite, kindly lent by the superintendent of the Coast Survey, Prof. A. D. Bache, was furnished with repeating circles; the diameter of the horizontal circle was six inches, with the limb divided from five to five seconds, and provided with two verniers; the vertical circle has four verniers, and is of the same size and graduation as the horizontal circle. The value of one division of the level was found to be 1″.13. This instrument was much injured by a fall in the water and rendered unfit for use by a second accident, two months later, in November, 1853, when it fell from the pier at the observatory. The following tabular statement contains the dates, nature of observations, and instruments used:—

VAN RENSSELAER HARBOR.

Sept. 12, 1853.	Observations of the sun's zenith distance for time, one set; Gambey theodolite.
" " "	Observations of the circummeridian altitudes of the sun for latitude, two sets; Gambey theodolite.
Feb. 20, 1854.	Observations of double altitudes of Saturn for time, two sets; Gambey sextant.
May 14, 1854.	Observations of circummeridian altitudes of the sun for latitude, two sets; Gambey sextant.
" 15, "	Observations of circummeridian altitudes of the sun for latitude, two sets; Gambey sextant.
" 16, "	Observations of circummeridian altitudes of the sun for latitude, two sets; Gambey sextant.
" 16, "	Observations of equal altitudes of the sun for time, two sets; Gambey sextant.
" 17, "	Observations of equal altitudes of the sun for time, two sets; Gambey sextant.
" 17, "	Observations of circummeridian altitudes of the sun, for latitude, two sets; Gambey sextant.
" 19, "	Observations of equal altitudes of the sun for time, one set; Gambey sextant.

The pocket chronometer was set to indicate Greenwich mean time within a few minutes; we have, for instance, from the above observations:—

		h. m. s.
May 16.	Pocket chronometer fast of Fern rock mean time	4 39 24.9
" 17.	" " " " " "	4 39 24.4
" 19.	" " " " " "	4 39 21.8
" 20.	Observations of circummeridian altitudes of the sun for latitude, Gambey sextant.	

RECAPITULATION OF RESULTS FOR LATITUDE OF VAN RENSSELAER HARBOR.

Sept. 12, 1853. For the observatory 78° 37′ 44″, which result has been used for the reduction of the transit observations at the observatory. The following results for latitude refer to the position of the brig Advance some distance to the southward of the observatory:—

May 14, 1854	78° 37′ 11″	mean 78° 37′ 4″ ± 3″.
" 15, "	36 58	
" 16, "	37 06	
" 17, "	37 12	
" 20, "	36 55	

which result was used for the reduction of the occultation and eclipse observations; it gives also the latitude of the meteorological observatory on the floe.

The observations for longitude of Van Rensselaer harbor consist of transit observations for time and of the moon and moon-culminating stars; also of occultations and an eclipse observation.

The transit observations commence November 18, 1853, and end January 10, 1854. Time was noted by the pocket chronometer. The transit instrument was supplied with five wires and are recorded from I. to V. in the order in which the star (or moon) passes them at the upper culmination, the circle being east of the telescope. The letter R, attached to the name of the object observed, indicates that its transit was observed reflected from a mercurial horizon: this method of observing became necessary for the measure of the inclination of the axis in consequence of the intense cold affecting the length of the bubble to such a degree that the level became useless. At temperatures below —40° no use could be made of the instrument. It was properly adjusted — an operation of some difficulty in so high a latitude and at so low a temperature. For the azimuthal adjustment there remained but an arc of 11½° between the pole and the zenith. The instruments,

when composed of two metals, become difficult to manage in consequence of unequal contraction. A peculiarity in the construction of the instrument requires to be noticed; it does not permit direct observation of a star elevated more than about 50° above the horizon, hence all observations upon the pole-star and others near the zenith had to be observed reflected. Transit observations were made on the following dates:—

Nov. 18, 1853	Moon and 8 stars.
" 21, "	" " 10 "
" 23, "	" " 11 "
Dec. 8, "	" " 4 "
" 9, "	" " 2 "
" 12, "	" " 4 "
" 13, "	" " 9 "
" 14, "	" " 6 "
" 15, "	" " 12 "
Jan. 8, 1854	Moon, Saturn and 7 "
" 9, "	Moon " 5 "
" 10, "	" " 4 "

For the reduction I have adopted lat. 78° 37½′, long. 4h. 43m. 28s. W. of Greenwich. The reduction was made by application of the method of least squares; there being no level readings, the amount of inclination of the transit axis had to be found from the transit observations themselves; the number of unknown quantities in the normal equations was reduced to three, since the collimation error could be deduced independently. The instrument was adjusted for collimation on November 18 and on December 13. Polaris was observed, reflected, circle east and west; the result was, however, satisfactorily checked from those sets of observation including stars above and below the pole. A preliminary reduction was made in order to ascertain an approximate value for rate of the chronometer; the observations were corrected for rate. For the reduction of the incomplete transits and for deducing the collimation error, the equatorial intervals of the wires have been deduced from transits of eighteen stars, as follows:—

For Circle East and Upper Culmination.

	s.	
I.	—39.71	The probable error of each interval is on the average ±0.07s.
II.	—19.82	
III.	— 0.22	
IV.	+19.84	
V.	+39.91	

The immediate purpose of the reduction is to obtain values for instrumental deviations, the necessary corrections have been applied to the observed transits of the moon and moon culminating stars; it is, however, only the difference of these corrections which affects the resulting longitude from the moon culminations. For the first three observations of Nov. 18, 1853, we have no means of ascertaining the level and azimuthal deviation, the corresponding factors, however, for the moon and stars are nearly equal.

RECAPITULATION OF THE DEDUCED SIDEREAL TIMES OF THE MOON'S LIMB.

			h. m. s.				h. m. s.
1853.	Nov. 18	☾ II. s. p.	17 57 43.2	1853.	Dec. 13	☾ I.	4 19 11.1
"	" 21	☾ II. s. p.	20 40 00.7	"	" 14	☾ I.	1 50 53.8
"	" 23	☾ II.	10 49 25.0	"	" 14	☾ II.	5 13 06.4
"	Dec. 8	☾ I.	0 23 31.5	"	" 15	☾ II.	6 06 38.4
"	" 9	☾ I.	1 09 53.8 ?	1854.	Jan. 8	☾ I.	3 13 14.5
"	" 12	☾ I.	3 29 35.6				

The longitude is deduced from the above values by a method received from Prof. Peirce in 1851, an account of which is given in Coast Survey Report for 1858, Appendix No. 21. From the Greenwich observations corrections were deduced to the tabular places of the moon's right ascension.

RECAPITULATION OF RESULTS FOR LONGITUDE FROM MOON CULMINATIONS.

			h. m. s.
1853.	Nov. 18	☾ II. s. p.	4 45 08
"	" 21	☾ II. s. p.	4 43 32
"	" 23	☾ II.	4 43 53
"	Dec. 8	☾ I.	4 47 57 ?
"	" 12	☾ I.	4 45 39
"	" 13	☾ I.	4 43 39
"	" 14	☾ I.	4 43 38
"	" 14	☾ II.	4 43 14
"	" 15	☾ II.	4 43 58
1854.	Jan. 8	☾ I.	4 41 50

The value of Dec. 8 must be rejected on the ground of imperfect transits. The result of Dec. 9 is unreliable and has been rejected.

The weighted mean from nine observations is 4h. 43m. 34s. ± 13s. If we combine the results according to the moon's limb we find:

	h. m. s.	
from ☾ I.	4 43 14	weight 8.5.
☾ II.	4 43 50	10.5.
adopted value	4 43 32	

RECORD AND RESULTS OF THE OCCULTATIONS AND AN ECLIPSE OBSERVED AT VAN RENSSELAER HARBOR.

In the record the times are given by chronometer *uncorrected* for error and rate, in the two accounts published in Appendix No. IX. of the Narrative, and No. 1017 of the Astronomische Nachrichten, the time is mean local time, as made out by Mr. Sontag. Observations were made on the following dates: —

Occultation of Saturn Ring, ...	Dec. 12, 1853.
" of Saturn Ring, ...	Jan. 8, 1854.
" of Saturn Ring and centre, . .	Feb. 4, 5, "
" of Mars, ...	Feb. 13, "
Eclipse of the Sun, ...	May 15, 1855.

The method of reduction used is that of finding the time of true conjunction in right ascension (for which see Sawitsch's treatise on Practical Astronomy, German edition by Dr. Goetze). The tabular nautical almanac places and data have been corrected, when practicable, from the Greenwich observations. The contacts of the ring of Saturn were referred to the centre by application of a radial correction deduced from diagrams of Saturn and ring, and the relative positions of the moon. Conditional equations were formed in each case, and the following results were reached: —

		h. m. s.	
Dec. 12, 1853,	Occult. of Saturn,	4 43 51.8	
Jan. 8, 1854,	" "	4 43 31.0	
Feb. 4, 5, "	" "	4 43 27.6	
Feb. 13, "	" Mars,	4 43 01.2	
May 15, 1855,	Eclipse of the Sun,	4 43 41.9	
Mean,		4 43 30.7 $\pm$ 6s.	
Resulting longitude from 9 moon-culm's.		4 43 32.0 $\pm$ 13	
Final longitude of winter quarters,		4 43 31 $\pm$ 7	W. of Greenwich.
		$= 70° 52' 45'' \pm 1\frac{3}{4}'$	

If we compare any of the separate results for longitude with the final value, and in considering the probable errors, it should be remembered that one degree of longitude in parallel 78° 37′ is but 11.88 nautical miles, thus the above uncertainty of 7s. in the final result for longitude, is but half a mile of linear measure.

The following table contains the result of the reduction of the remaining astronomical observations, and of some adopted longitudes: —

	Lat.	Long. W. of Greenwich.		
Fiskernaes,	63° 02′.8	50° 32′.5	3h. 22m 10s.	(approx.)
Proven,	72 23 .0	55 37 .5	3 42 30	"
Upernavik,	72 46 .2	56 02 .8	3 44 11	by Inglefield.
Refuge harbor,	78 32 .7	73 50	4 55 20	
Cape Inglefield	78 34 .1	72 55	4 51 40	
Marshall Bay	78 51 .1	68 54 .0	4 35 36	

The following positions were determined from observations by travelling parties: —

	Lat.	Long. W. of Greenwich.
Cape John Frazer, . . .	72° 42′.9	71° 30′
Cape Prescott, . . .	79 35 .2	72 56
Cape William Wood, . .	78 59	68 20
Cache Island, . . .	79 12 .5	65 30
Cape Andrew Jackson, .	80 01 .6	66 52
Cape Jefferson, . . .	80 41 .2	67 52
Cape Madison,. . . .	80 20 .2	66 52
Littleton Island, . . .	78 22 .0	74 10
Cape Alexander, . . .	78 09 .3	74 20

The following results are taken from a report of Mr. Sontag's to Dr. Kane, dated Sept. 12, 1855 (at Godhavn): —

	Lat.	Long. W. of Greenwich.
Fitzclarence Rock, . .	76° 55′.0	———
Dalrymple Rock, . .	76 30 .5	70° 23′
Parker Snow point, .	76 04 .2	68 44
Cape York, . . .	75 56 .0	66 48
Godhavn, . . .	69 14 .6	———

The complete paper, as printed by the Smithsonian Institution, is accompanied by a newly-projected map. Comparing the same with the original chart in Dr. Kane's Narrative, it will be perceived that the only change of importance made is the shifting of the shores of Kennedy Channel to the southward to an amount of about nineteen nautical miles; it is well known that Dr. Kane had adopted the mean positions resulting from astronomical observations and dead reckoning, whereas in the new map accompanying the astronomical paper, the astronomical results alone have been used. This change was made with the concurrence of Prof. Bache, who, in May, 1858, communicated to the Royal Geographical Society in England, that such a step seemed desirable and proper. The highest point of the shore line, traced by Morton, on

the east side of the channel, is now placed in latitude 80° 56′, and, on the opposite side, the highest point distinctly seen by him, is located in latitude 82° 07′. Mr. Sontag also expressed himself favorable to this change made in the latitudes of the highest land seen.

Some observations were made by Dr. Kane on the twilight at Van Rensselaer harbor, in latitude 78½°, the limits of legibility, for ordinary large type, were with a depression of the sun below the horizon of 7° 26′ and 8° 11′. It is generally assumed that in temperate latitudes, complete darkness sets in when the sun's depression reaches 18°; on the 2d of March, the first appearance of twilight was noticed at a depression of 15° 0′ (temperature — 37°), in lat. 78½°; thus it appears that in this high latitude twilight is more feeble with the same depression of the sun than in lower latitudes. This circumstance is, doubtless, owing to the diminished height of the atmosphere (by contraction, on account of the cold, and by compression), in the high latitudes.

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5. On a New Theory of Light, proposed by John Smith, M. A. By Prof. Ogden N. Rood, of Troy, N. Y.

Several months ago, when attempting, by means of a revolving disk, to measure the time occupied by the explosion of small charges of gunpowder, the following observation was made: The flame of a burning-fluid lamp was viewed through a rotating disk provided with four radial slits, and it was found that a certain rapidity of rotation caused the lower part of the flame to assume a green hue, while by a diminished rate the whole flame was colored deep purplish red; a lower rate gave a violet tint, alternating with pure white.

It was evident that these appearances depended much on the state of the eye, for they often could be perceived only after it had become a little fatigued by the blinding effect consequent on the comparatively slow succession of the impressions of light; in addition to this, I found that a friend who was present, though recognizing the green tint, was unable to perceive the red hue, with which my eyes at that very moment were dazzled.

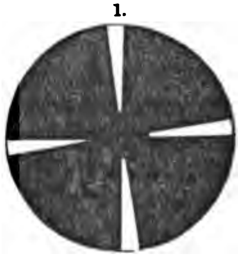
I considered these appearances, therefore, as *subjective*, and laid the matter aside for future experiment. As, however, similar phenomena

have been observed in England, and have been thought to have not only an objective existence, but to be worthy materials on which to build a new theory of light, it may not be amiss to enter into a slight examination of their nature.

In Mr. Smith's experiments, of which I have been able to obtain only the account given in the March number of the American Journal of Science and Arts, bright white light is allowed to act on the eye during a certain fraction of a second; it is succeeded by shadow or darkness, which lasts also during a certain short interval of time, when the operation is repeated anew, &c.

This pulsation of light and shade the author effects in a variety of ways; the result is color — a yellowish green, purple, pink, &c. Fechner, to whom we are indebted for extensive researches on sight, several years ago observed that white disks having black spiral figures painted on them, when set in rotation exhibited colors which he considered subjective.*

1.



That these colors are really subjective, the following simple experiment may serve to show: A blackened disk nine inches in diameter, was cut with four slits of the shape seen in the woodcut; the width of the slits at the circumference was $\frac{5}{10}$ of an inch; the disk was made to rotate before a bright cloud. A rate of ten revolutions per second caused the cloud in a short time to appear of a deep red color, having in it a tinge of purple, or, according to Mr. Smith, the disk transmitted pink light; it was now viewed through a plate of orange-tinted glass. Previous experiment with a small telescope provided with a micrometer, and a flint glass prism, had shown that this orange-colored glass readily transmitted the red, orange, yellow, and a portion of the green, but that it was opaque to the blue and violet rays; it was therefore fairly to be expected that if the disk was really transmitting red light, the plate of glass would do the same. The result was different; through the glass the disk appeared of a bright greenish-blue color. This experiment is very easy to make, and the effect is brilliant.

Plates of glass of other tints were now employed; the results are given below.

* Pogg. Ann. Vol. XLV. p. 227.

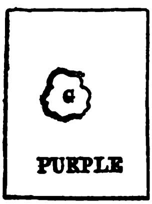
Medium.	*Tints of the Disk.*
Yellow glass,	Violet,
Green "	Green, neutral, or faint red,
Red "	Red, neutral, or faint green.

As the green glass was nearly opaque to red rays, the effect of its use ought to have been darkness. It is evident by an inspection of this table that the disk really transmitted *white*, and not red light, which becoming colored by its passage through the plates of glass, induced in the retina, from time to time, the sensation of the complementary tint, more or less mingled with the *original* impression.

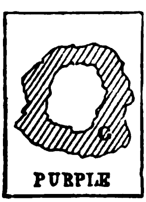
Having now shown that, contrary to Mr. Smith's supposition, the light transmitted by the revolving disk is really white, let us notice some of its effects on the eye.

For this purpose I caused perforated disks to revolve at uniform rates by means of clock-work; the arrangement being similar to that employed by Plateau. A blackened disk five inches in diameter and perforated with four slits 7° 12′ in the width was set in rotation, and the bright sky viewed through it; the eye of the observer being immediately behind the disk. With a rate of 11½ revolutions per second, the appearance of the window was as in fig. 2; a central spot was colored bluish green, the rest of the field was purple, or reddish purple, according to the state of the eye. The green spot remained always in the axis of vision, and moved with each change of it. With the exception of fluctuations in the outline of the spot, this appearance remained tolerably constant, as long as the rate of revolution continued the same. The spot or shadow was fringed with a narrow, faint blue border, indicated by the dotted line.

2.



3.



Upon increasing the rate of revolution, the bluish green spot expanded into an irregularly shaped ring, and continued to expand, filling the field, till the rate had become as high as 15 rotations per second, when often the field for an instant became of a greenish tint which was succeeded by a bluish tint; upon increasing the speed this also vanished. Still higher rates cease to produce any of these peculiar effects on the eye.

Upon slowly reducing the rate to 9 revolutions per second, the green spot contracted in dimensions, and assumed a yellow tint, while the

field often became at the same time tinted deep crimson. With a rate still lower, the appearance of the field is variable and the tints flickering; it assumes sometimes a purple, a yellow, or a yellowish-green tint.

This experiment I repeated a great number of times, with the same general result, and though it sometimes happened that the eye became insensible to these colors, from repetition, momentary rest in darkness restored this power for a short time. Thus it occurred that the tints were sometimes seen with great distinctness, while at others they could hardly be distinguished.

Upon a dark cloudy day to produce these effects it was found necessary to increase the width of the slits to 20°; from whence it was manifest that lack of intensity in the light might be made up by its longer duration.

It would appear then, from these experiments, that light from a bright cloud, if allowed to act on the eye repeatedly during from $\frac{1}{450}$ to $\frac{1}{750}$ of a second, develops subjective colors; that, however, the development of the subjective tint is dependent not so much on the length of time which the eye is exposed, as upon the interval of *rest* or shade which follows each exposure, may be shown in the following manner: In the experiment where with $11\frac{1}{2}$ revolutions a reddish purple was produced, the exposure lasted $\frac{1}{575}$ of a second; the interval of rest or shade was $\frac{1}{50}$ of a second; now a disk was cut similar to fig. 1, but having eight slits, each 7° 12′ in width, when it was found that 5.5 instead of 11.5 revolutions per second produced the purplish red tint; here the exposure was twice as long, but the interval of rest or shade nearly the same. With sixteen slits, $2\frac{8}{10}$ revolutions produced the same tint, the exposure being of course four times as long, but the interval of rest nearly the same. Determinations of the length of this interval are given below: —

Length of the intervals of shade required for the production of

Blue, ...	$\frac{1}{65}$ sec.
Purplish red, ...	$\frac{1}{50}$ "

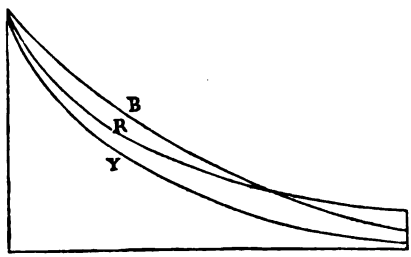
To ascertain exactly what portions of these intervals elapse before these tints make their appearance, or how long the tints are actually seen by the eye, is another matter, and would no doubt require an elab-

orate series of experiments, though it would seem probable that at least half of the time of the above given intervals passes before the subjective color makes its appearance.

The table does not apply to the axial portion of the retina, which is almost always differently affected. That the change in susceptibility in the retina is progressive outwards, is shown by the gradual expansion of the green ring; that it varies from second to second, is seen in the fluctuations of the outline of this ring.

The occurrence and sequence of these subjective colors may easily be explained by supposing that during the interval of rest or shadow the action of the yellow rays diminishes more rapidly than that of the red, the red more rapidly again than that of the blue. If this takes place as indicated by the curves below, it is easy to understand the production

4.

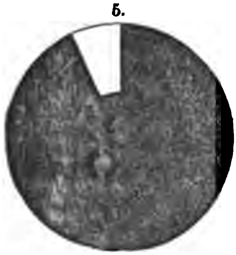


of the tints, for if the moment after the blue has been developed white light be again presented to the eye, it is evident that neither purple nor red will be seen, sufficient time not having been allowed for their production.

The same reasoning applies to the axial portion of the retina, which, owing to its greater sensibility to such impressions of light, requires a somewhat longer interval of rest before the reaction occurs.

If the impression be too strong, that is, if the light be too bright or the eye too long exposed to it, these peculiar effects are not observed, and during such short intervals of rest as $\frac{1}{50}$ or $\frac{1}{80}$ of a second the

5.



white clouds seen through the disks suffer no change in tint; but if a blackened disk twelve inches in diameter be cut as seen in the figure, with an aperture of 30°, and made to rotate before a white cloud at a rate of only one revolution per second, the eye placed as near it as possible, will most distinctly see, in the interval of darkness, an image of the sky, of a bluish green tint. Whence it follows that an exposure of the eye to white light lasting $\frac{1}{12}$ of a second, induces in it for a considerably longer time the sensation of this color.

In general these experiments seem to point out that after momentary exposure to white light subjective colors are induced in the eye, whose tint and duration are dependent on the strength of the impression received as well as upon the length of time allowed for rest; these sensations of color apparently having a relation to the colors observed after looking at the sun, similar to that which a temporary disorder bears to a chronic affection.

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6. Registering Thermometer. By James Lewis, of Mohawk, New York.

The experiments preliminary to the construction of the apparatus now about to be described, were made during the fall and winter of 1859, and completed by the construction of a registering thermometer, which gave satisfactory assurances of utility as early as the middle of March, 1860. A slight defect in parts of the apparatus, however, rendered it necessary subsequently to make alterations, in order to secure uniformity of action and freedom from errors. The record of the apparatus may be said to commence on the 6th day of May, 1860, and has been continued since, to the time of writing this description.

While yet the apparatus was incomplete, and gave promise of utility, brief notices of it were presented by Mr. Lea, of Philadelphia, to the Academy of Natural Science and the American Philosophical Society, and a notice was also sent to the Boston Society of Natural History.

More recently it has been thought expedient to prepare a full description of the apparatus, with details of its operation. (These are herewith presented.)

The first appearance of the apparatus presents the following features: A simple wooden case about twenty inches high and 7×7 inches lateral dimensions, has a dial near the top of the front, with a balanced index pointing to a scale on the dial graduated after Fahrenheit from $-30°$ to $+110°$, a small portion of the circle remaining blank. On the left hand side of the exterior of the case is seen a bundle of polished metallic rods, brass and iron, protected from accidental contact with external objects by other rods around them, and as a precaution

against radiation, between the rods and the wooden case a plate of polished metal (tin) is interposed.

After opening the front of the case, and removing the dial and index, also the various protections around the rods on the outside of the case, the apparatus presents the following general appearance:—The several parts thus exhibited occupy nearly one half of the depth of the case from front to rear, being separated from the posterior half of the case by a vertical partition, to which all the parts here represented are attached. The parts which are enclosed in the posterior apartment of the case are a series of clock movements and cylinders of paper which will be only partially explained, as they are within the scope of any ordinary mechanic, and refer simply to the mechanical accessories of the recording apparatus.

The thermometer, which is the essential feature of this apparatus, consists of a bundle of iron and brass rods, arranged around a centre for the purpose of connecting them with each other at equal distances from, and as near to the centre as practicable, in order to obviate any elasticity in the connecting parts: also for the purpose of enabling each individual rod in the bundle to contribute its stiffness to the whole in a manner to secure the greatest rigidity and a constant parallelism of the rods. The arrangement of the rods is that which is illustrated by the compensated pendulum. The whole number of rods or wires in the bundle is eleven, six of these, in three pairs, are iron, and on these the force of compression is used. The force of extension is applied to four brass wires; two pairs are engaged alternately with the iron wires around the centre, and also a single brass wire in the centre, which single brass wire is the equivalent of a pair. Thus the first pair of iron rods is directly connected with the base of the thermometer, and the last (single) brass wire with the terminus. These wires are connected in the proper order by means of circular disks, corresponding to the cross-bars of the gridiron pendulum, there being three of these disks at the upper and three at the lower end of the instrument. The upper extremities of all the rods are screwed to the disks to which they are united, passing freely through the other disks with which they are not united; the lower ends of the *iron* rods have a modification of the tenon and mortice in their connection with the disks to which they are united, and their union is made permanent by means of tinner's solder. The lower ends of the brass rods are

secured to their respective disks by a nut holding against the lower side of the disk. The outer portion of the circular bundle is composed of three pairs of iron rods and two pairs of brass rods. The centre of the bundle is occupied by a single brass rod, which is secured at its lower extremity by means of a thumb-screw, which, as will hereafter be seen, serves for purposes of adjustment.

The base of this system of compensated rods is the disk at top, having a conical projection on its upper surface; this conical projection articulates with a portion of the metallic base, to which the various levers within the case are attached. This articulation is a modification of the ball and socket joint. The part of the large brass plate with which this articulation is made is turned at a right angle with the main body of the plate, in a horizontal direction, and serves, partially, also, to sustain the parts upon which the fulcrum of the large lever rests. By following the various connections of rods and disks from the base of the system of rods (the disk having the conical projection) our attention is drawn to connections of iron and brass, alternating, the last brass rod being the centre rod of the bundle, the upper extremity of which rod is screwed into a jointed appendage of the large lever. In the apparatus from which this description is made, the iron and brass rods are No. 13 wire, and present in a vertical space of about fifteen inches the equivalent of $45\frac{8}{10}$ inches of iron wire antagonized by an equal length of brass wire. The difference in the rates of expansion of these two metals thus united, under the influence of plus and minus quantities of heat, are sought to be used in this apparatus for the measurement of variation of temperature.

The bundle of rods as above described and illustrated are secured permanently in a pendent position, which was thought to be the best arrangement which could be effected, as thus a very slight assistance is derived from gravitation in securing their best action.

To preserve the parallelism of the rods with the case enclosing the other portions of the apparatus, a bracket made of brass (or any suitable metal) is made to embrace loosely the first pair of *iron rods* of the bundle, and is then screwed to the side of the case. In the construction of such a bundle of rods, it is necessary to secure free vertical movements of all the parts not necessarily united with each other firmly, perfect parallelism, and a homogeneous character for each of the two metals used. It is also necessary that the rods be brightly polished

and varnished to preserve their brightness, in order that the sucepti-bility of the rods to radiation may be reduced to the lowest possible amount. It is very uncertain whether a polished metallic plate inter-posed between the rods and the case for the purpose of intercepting radiation between them is of any use or not. But to preserve the rods from accidental blows, which might disarrange them, it is necessary they should be protected by some arrangement outside of them which will answer this purpose, while it permits a free circulation of air around the rods.

The construction and use of the rods being now clearly set forth, their connection with a system of levers will be explained. By means of the central brass wire in the bundle, passing upward through the several disks and the conical projection of the upper disk (without con-tact with any of them), the movements of the compensated rods are communicated to a jointed appendage of the large lever. This ap-pendage is made with a joint in order to insure equal action of its two sides on the two ends of the steel knife edge which terminates the short arm of the lever. This lever multiplies the movements imparted to it eight times, and communicates them to another lever by means of a jointed appendage, rod and chain made of flat links and rivets. This lever has its two extremities made in a circular form, in order to avoid some errors in conversion of rectilinear to circular motion. (The movements of the *large* lever not being so considerable as to differ sensibly from rectilinear motion, it is not necessary it should have this form.) The second lever multiplies the movements imparted to it five times, and communicates them to the smaller portion of a pulley by means of a flat-linked chain. The chains used for these connections should be made with special reference to avoiding friction, and with this view the several pieces of metal forming the links should have one end a little wider than the other, and should be arranged so that the wider end should be the last to come in contact with the convex sur-faces on which they are flexed. The rivets should not be so firmly headed but that each link will turn on its rivets by its own weight.

The pulley multiplies the movements imparted to it eight times, and communicates them by means of a slender, varnished silk cord running in its channelled periphery, to a light metallic frame carrying a register-ing point. This frame has two lateral arms. The arm on the left divides into two springs, the anterior one carrying the registering point,

the posterior one being perforated near its extremity in order to permit the *point* to pass through it; the arm projecting on the right is simply a counterpoise, by means of which the *frame* is secured in a perpendicular position. The design of this is, to avoid *friction* between the frame and two slender parallel rods which serve to direct the movements of the frame and registering point, in one uniform perpendicular path. These parallel rods are made of very fine brass wire. They are in the same vertical plane, one immediately in front of the other. The ends of these wires have heads melted on them by means of a common blowpipe, which heads serve to secure them at their upper extremities to a stationary bracket which projects from the large metallic plate which forms the base and support of the levers, &c. The lower ends of these wires are held by two springs which serve to preserve a uniform tension, under varying temperatures, so that the wires shall remain parallel. The size of the wires is No. 32 (wire measure).

In reviewing the general arrangement of the several parts of the thermometer which have been noticed, a few other particulars are especially deserving of attention. The several *knife edges* of the levers should rest on agate. The axis of the pulley should rest on friction wheels, and should be so constructed that it could not come in contact with any thing but the friction wheels. The levers and pulley should be made as light as possible, and their forms should be such as would give the greatest strength with the least amount of material. No very great strain at any time comes on any of the parts, but elasticity and friction should be avoided at every point. The greatest amount of strain is felt on the short arm of the large lever, and on the bundle of rods. This will be 320 times the weight of the frame that carries the registering point, diminished a little by the second lever, and increased somewhat by the long arm of the first lever. The pulley should (when divested of the chain and silk cord) be so evenly balanced as to remain perfectly at rest, in whatever position it may be placed, while resting on the friction wheels. The metallic plate (not shown in diagram) which supports the pair of friction wheels which sustains the anterior extremity of the shaft of the pulley, has a tube or hollow shaft projecting from its front surface, through which the anterior extremity of the axis of the pulley projects for the purpose of accommodating the *index* spoken of at the commencement of this paper. This tubular projection serves as an axis for the support of the *dial*, and permits the dial to be

turned to any position to correspond to the index, when an adjustment is necessary.

The index should be so perfectly balanced that when connected with the pulley divested of its chain and cord, the balance of the pulley may not be impaired.

The apparatus thus far described is simply the thermometer, susceptible of indicating temperature with exceeding delicacy. To *record* indications of temperature is the object of that portion of the apparatus not yet described.

Immediately behind the path of the registering point is a cylinder nearly $\frac{3}{4}$ of an inch in diameter, the axis of which is vertical, and parallel with the path through which the registering point moves. This cylinder presents in close proximity to, but not in contact with, the registering point, a fillet of paper $5\frac{5}{16}$ inches wide, on which to receive impressions from the registering point. The cylinder has a metallic shaft through its centre, around which is firmly wrapped a roll of soft paper which acts as a cushion, on which to receive the registering point without injury. The ends of the cylinder are protected by metallic heads which serve to keep the paper cushion from sliding up and down the shaft. The cylinder should turn freely on its axis, yet be restrained from making vertical movements however slight. In front of the path of the registering point is a hammer having a long, narrow face parallel with the cylinder. This hammer is connected with its axis by means of three metallic bars. The striking face of the hammer is of sufficient width to cover the head of the registering point, and its length is a little less than the length of the cylinder, so that it can in no case drive the registering point against the metallic heads which form the ends of the cylinder.

The movements of the hammer are controlled by two springs. One of these is connected with the lever, on the upper end of the hammer axis, and operates to cause the hammer to strike the registering point when released after being drawn back. The other spring serves as a recoil, to keep the hammer from contact with the head of the registering point while the hammer is at rest. A square-headed screw (with a key), is used for adjusting the position of this spring.

In connection with the hammer should be a *point* (which should not be placed so as to interfere with the registering point), for producing a row of marks along one margin of the fillet of paper on which the

record is made, which row of marks will serve as a *point of reference*, or a substitute for zero, when it is desired to ascertain the numerical values of the records on the fillet of paper. The apparatus from which this description is made has a device of this kind, which marks the paper at intervals of one hour. But as this is merely a matter of convenience for temperature instead of time, a simple steel point $\frac{1}{8}$ of an inch to the left of the face of the hammer, and fastened to one of the three supports of the hammer will answer the purpose, — if it be so placed as to make a mark on the paper every time a record is made by the hammer impinging on the registering point.

The axis of the hammer, like that of the cushioned cylinder, should be restrained from all vertical movements, as any movements of this kind would make the *reference marks* irregular and likely to prove to be a source of errors.

The cylinder being filled with paper, is introduced within the rear compartment of the case. In this position, it is held by a *brake* which supports on the extremity of a lever at each end of its axis, a smaller cylinder, over which the paper passes in its progress to the cushioned cylinder. This small cylinder being at the extremity of a lever serves to release the brake which holds the first cylinder when the paper is drawn tightly. After passing from *left* to *right* around the *front* of the cushioned cylinder, the paper is embraced between this and a *feed* cylinder of small dimensions behind the cushioned cylinder (or in front, if we look at it in the rear compartment of the case), and after passing partly around the feed cylinder, the paper is finally wound up by the receiving cylinder. A connection is made between the lever on the upper end of the hammer axis, and a rock shaft placed horizontally above the ends of the train of cylinders in the rear compartment of the case. This rock shaft serves as a means of communication between a train of clock-work and the hammer: it also serves to actuate the *feed* cylinder by direct movement on a ratchet wheel, and also serves to actuate the receiving cylinder, by releasing a spring which imparts the necessary and *variable* movement of the receiving cylinder. The clock work connected with this apparatus is in two parts. In the upper part of the rear compartment above the rock shaft and cylinders, is a train of wheels actuated by a powerful spring, the whole duty of which is to move the hammer, and feed the

paper through the cylinders. No ordinary clock movement has sufficient strength in its striking spring to be available as a combined time and recording clock, without requiring too frequent winding of the recording spring.

In the lower portion of the rear compartment, is a small marine clock with jewelled movement to insure action in cold weather. On the shaft of the minute hand of this clock is secured a disk or wheel, having four pins of unequal length projecting parallel with the shaft. These pins act upon a lever which detaches the movement of the *recording clock* every quarter hour. A wire connects the two clocks through this lever. This lever is so formed that it may be made to come in contact with one, two, or all of the four pins which act upon it, the pins being made of different lengths for this purpose.

More attention has been bestowed upon the thermometrical portion of this apparatus, than upon the other portions (both in construction and in description), for the reason that that portion is the essential part without which the other parts are useless. The thermometer and the method by which its indications are recorded, do not seem to be susceptible of much improvement beyond what is here said of it. The accessories of the recording apparatus, the manner of arranging the cylinders and clock work, are susceptible of various modifications, some of which would no doubt much improve the apparatus and render it more easy of control than as described. Yet, in its present form, no very great difficulties are experienced in its use.

The construction of the apparatus being sufficiently explained to render it easy for any one of sufficient mechanical ability to reproduce a similar apparatus, a few words in regard to its operation, and other matters incidental thereto, seem necessary.

The apparatus as described, presents a range of about 98° for the movement of the registering point against the paper on which the record is made. The limited range of record, renders it necessary that *two adjustments* should be made yearly, where the extreme range of temperature exceeds 98°. In this latitude, one adjustment is necessary in spring for maximum temperature, another in fall for minimum temperature.

The apparatus is adjusted as follows:

Firstly, the registering point is adjusted by turning the thumbscrew at the bottom of the bundle of rods, until the registering point corresponds to the middle of the cushioned cylinder — or opposite the central bar that connects the hammer and its axis. Secondly, the index is adjusted to point perpendicularly upwards — the registering point being undisturbed. A few observations extended daily for a week, by comparison with a reliable mercurial thermometer, will give an approximate scale for the diagram of temperature made by the registering point. After a few corrections by successive comparisons, the scale for the diagram may be made free from errors of appreciable magnitude; after which the scale for the dial may be plotted from the scale used for the record, allowance being made for the difference of the radius of the pulley and the radius of the circle on which the scale is to be plotted on the dial.

The operations of the apparatus from which this paper is drawn, are not so perfect as they should be for several reasons. The mechanical conveniences at hand during the process of its construction were of a very rude character, the various parts not being so free from friction and other sources of error of a mechanical nature as they could be; among the most serious causes of error, is the want of agate bearings for the knife edges; the rude form of the *links* of the *chains* connecting the levers and pulley; and a considerable amount of friction between the rods and the disks through which they were intended to pass freely. Much of this friction, no doubt, could be lessened by reducing the number of points of contact to a minimum, and having the surfaces in contact burnished. Yet, with all these sources of error, the greatest error involved in the record of the apparatus from these causes, is scarcely more than $\frac{1}{10}$°. The fortunate necessity for a *weighted lever* to connect the time and recording clocks, remedies nearly all the errors of friction for about $1\frac{1}{2}$ seconds before each record is made, a weight of half an ounce falls about one inch, producing vibration, which is sufficient to establish an equilibrium. This vibration is assisted somewhat by the further vibrations produced by the wheels of the recording clock while the hammer is being raised to make a record.

In the operation of this apparatus, constant discrepancies have been observed between the indications of the apparatus and a mercurial thermometer hung by the side of the rods. These discrepancies were for a long time the fruitful source of perplexity — and sometimes were suf-

ficient to lead to a suspicion that the mechanical principles involved in the apparatus had not been carefully and judiciously applied. It was afterwards discovered that these discrepancies were greatest under circumstances extremely favorable to radiation, and less so under circumstances where radiation was guarded against. A careful study of the phenomena of radiation in connection with the two instruments, eventually gave the most satisfactory assurance that *radiation* was the cause of the discrepancies.

Without going into all the details from which this conclusion was derived, a simple statement of some of the phenomena will be given.

In the early part of the day, while the temperature is rising, the mercury will precede the apparatus from $\frac{1}{10}$ to $\frac{7}{10}$ degree — according to the exposure to radiation. If very great exposure be made, the discrepancy may reach two or three degrees.

In the latter part of the day, when the temperature is declining, similar discrepancies appear, but the mercury in this case, is generally not so much in advance of the apparatus.

The explanation for these discrepancies seems to be this: In morning the sun's rays heat the objects on the earth's surface somewhat above the general temperature of the atmosphere. The black metallic frame in which the mercurial thermometer is encased, by proximity renders the mercury more sensitive to radiation than the polished rods of the apparatus are. And in evening, objects on the earth's surface loose this heat by radiation in space, and have a lower temperature than the air. The mercury being more sensitive (through its surroundings), to radiation, is in this case also more affected than the rods by the temperature of surrounding objects. But on the other hand, the rods are more sensitive to changes by *contact* than the mercurial thermometer, the smaller size of the rods, compared with the bulb of the thermometer, affording an explanation. The extreme sensitiveness of the rods to change of temperature by contact is exhibited in the fact, that on taking the apparatus and thermometer from a warm room (80°) in March to the cold air, (10°) the rods indicated a change of 20° in the same period that the mercury passed over 10°. The low temperatures of the atmosphere prevented further observations to ascertain how far this ratio was maintained.

In the description of the apparatus, the following remarks that would have been appropriate in their proper places were omitted.

The arrangements of the levers and pulley were selected as here described for the purpose of making a dial available, so that it would be at any time easy to compare the apparatus with a mercurial thermometer. The record as it appears on the cylinders is inverted; this is not a serious inconvenience, as persons who are accustomed to the use of inverting microscopes and telescopes can easily understand. The frame and arms connected with the registering point, should be no heavier than is necessary to keep the several parts concerned in its movements, in proper tension. Probably twenty grains is heavy enough for this purpose.

Without *agate bearings* for the knife edges of the levers, the zero point will gradually creep up the scale, and require careful watching and corrections. The amount of change on brass bearings is not far from one degree in about three months.

Method of using Apparatus.

In order to secure uniform results with this apparatus, and to avoid accumulations of errors in its record, it is necessary that it should be placed in a suitable station where the air circulates freely, while the rays of the sun are excluded, and all radiation intercepted as much as possible. The apparatus should be placed on a revolving pedestal, easily accessible, and never disturbed by any unnecessary vibrations or concussions. It should be so placed to afford easy access to either side of the apparatus without producing unnecessary disturbance.

It will be necessary to compare the working of the apparatus as often as is convenient, with a reliable mercurial thermometer. It may be done in this way. Before opening the front of the case, tap the case gently to be sure that the registering point is not in error from friction; then by pressing the hammer against the registering point with the finger, it may be held while the mercurial thermometer is inspected. The readings of the mercurial thermometer and dial may then be marked upon the record, and a line ruled along the hammer to point to the particular mark to which the readings refer. If discrepancies appear, the mean of an equal number of plus and minus mercurial differences will probably give the true data from which the readings of the apparatus should be corrected if any correction is to be made.

It will be found that in cloudy days in which there is no sunshine, and very little wind or a steady wind, there will be no sensible discrepancies. The discrepancies will also vanish near the points of highest and lowest temperature each day.

. . . . The record may be cut out of the apparatus daily for correction, copying and reduction. For this purpose it will be necessary to wind up a portion of the fillet on the receiving cylinder, until the *record* is all past the feed cylinder. The paper may now be cut, and unwound from the receiving cylinder and removed. The receiving cylinder should have a considerable piece of firm paper wound on it, for the purpose of pinning the unrecorded fillet to it. The unrecorded portion of the paper may now be wound back upon the first cylinder until only a slight fillet is left around the feed cylinder. This operation does not necessarily interfere with the record unless it be undertaken and not completed at a moment when a record is to be made.

The fillet of paper on which the record is made by the apparatus, should be soft and unglazed; if stiff, it is liable to embrace the registering point so firmly as to prevent the spring connected with it from detaching it from the paper.

The treatment of the record when taken from the apparatus is as follows: A fine line is extended over the sheet, along the marginal row of punctures used for a point of reference. This line is a guide by means of which the paper is pinned over a suitable fillet of firm glazed paper on a board used for the purpose, and the holes in the record made by the registering point are transferred to the paper below the record by means of a suitable pointed instrument; and afterwards each tenth degree of the scale of temperature may be plotted in its proper position in horizontal parallel lines, and the different hours of the record indicated by figures. A simple brass scale may be laid on the paper, from which the numerical values of the records may be read, even to tenths of a degree if necessary. A *vernier* for this purpose may at first be necessary, but a few trials are sufficient to enable one to dispense with the vernier.

It seems probable that the greatest value of this apparatus is in the assistance it will render in determining much more correctly, many meteorological questions, than can be conveniently accomplished by any methods heretofore employed.

For the purpose of determining mean temperature, records at intervals of one hour are frequent enough, as there is not much to be gained by computations from more frequent records; and if twenty-four records daily are to be used for computing mean temperature, they should be taken not at the beginning or end of each hour, but at the middle of each hour. It is not necessary, however, to discuss the reasons for this selection at this time.

Although records at intervals of one hour are sufficient for determining mean temperature, it is desirable to have *more numerous records*, for the reason that in this latitude in certain seasons of the year, the variations of temperature follow each other in numerous waves, having periods of an hour or less — and elevations and depressions of several degrees. From such a record means of intervals of one hour would be likely to involve errors which could be corrected by taking readings *from a line representing a mean of the waves of temperature.*

II. PHYSICS OF THE GLOBE.

1. ON THE OPTICAL PHENOMENA PRESENTED BY THE "SILVER-SPRING" IN MARION COUNTY, FLORIDA. By Prof. JOHN LE CONTE, of Columbia, South Carolina.

THE extraordinary reports in relation to the optical phenomena said to be exhibited by the "Silver-Spring," induced me — under the invitation and guidance of my hospitable friend, Col. A. G. Summer of Florida — to visit it during the month of December, 1859. And although — as might have been anticipated — many reputed facts vanished under the scrutiny of careful observation, and all of its so-called *mysterious and wonderful* phenomena are obviously referable to well-known physical principles; yet, it may be interesting to give a brief statement of them, and to indicate how they may be referred to the recognized laws of physics.

This remarkable "Spring" is situated near the centre of Marion county in the State of Florida, in latitude (about) 29° 15′ north, and longitude 82° 20′ west. It is about five miles north-east of Ocala — the county seat — and nearly in the *axis* of the peninsula, being

equally distant from the Atlantic and Gulf coasts. Its waters are discharged by a short stream bearing the same name (namely, "Silver-Spring"), which, running about six miles, unites with the Ochlawaha (or Ocklawaha), a tributary of the St. Johns river. The stream takes its origin in a *deep pool or head-basin*, which is called *par excellence*, the "Silver-Spring." This basin is nearly circular in shape, about 200 feet in diameter, and is surrounded by hills covered with Live-oaks, Magnolias, Bays, and other gigantic evergreens. The amount of water discharged is so large, that small steamers and barges readily navigate the Silver-Spring up to the *pool* or *head-spring*, where there is a *landing* for the shipment of cotton, sugar, and other produce. These steamers and barges make regular trips between the Silver-Spring and Palatka on the St. Johns. The boatmen informed me, that at its junction with the Ochlawaha, more than one half of the water is contributed by the Silver-Spring stream.

By means of a canoe, I explored the stream for about two miles from its head: its *breadth* varied from forty-five to 100 feet, and its *depth*, in the *shallowest parts*, from ten to fifteen feet: — the *smallest depth* measured was not less than ten feet in the channel of the stream. The *average velocity* of the stream was rudely estimated at about two miles per hour: at the *narrowest portions* it was estimated at three miles per hour. According to the reports of the residents, the level of the water of the Spring at the landing, is subject to comparatively slight fluctuations; scarcely ever exceeding *two feet*. These fluctuations of level seem to be connected with the season of rains. The commencement of the rainy season varies from the 15th of June to 15th of July. The waters of the Spring begin to rise about the middle of the season of summer rains, and attain their maximum height about its termination.

The *maximum depth* of water in the pool or basin constituting the head of the Silver-Spring, was found to be not more than thirty-six feet in the *deepest crevice* from which the water boils up: — the general depth in the central and deep parts of the basin was found to be about thirty feet. These measurements were made by means of a heavy plumb-bob attached to a twine, to which bits of *white cloth* were secured at intervals of *six feet* or *one fathom*. As the plumb-bob as well as each piece of white cloth indicating the fathoms, could be *distinctly seen down to the very bottom*, the measurements were of the most satis-

factory character. Inasmuch as accurate quantitative determinations, however easily applied, are seldom resorted to by the unscientific, we need not be surprised, that the *real depth* of this Spring falls *very far short* of its *reputed depth!* In South Carolina, the reported depth was variously stated at from 120 to 150 feet; on the confines of Florida it was reduced to eighty feet; while the *smallest* estimate in the vicinity of the Spring was forty-five feet! This affords an illustration of the general law, that the *accuracy* of popular statements bears an *inverse* proportion to the distance from the point of observation; probably — like all emanations from centres — following the *law of inverse squares!*

Doubtless, the *greater portion* of the water which flows in the Silver-Spring river is furnished by this principal or *head-spring;* but there are several *tributary springs* of a similar character along the course of the stream, which contribute more or less to the volume of water. These usually occur in *deep basins*, or *coves* (as they are called), in recesses along the margin of the stream. The depth of one of these *coves* situated about 200 yards below the head-spring, was found to be thirty-two feet in the crevice in the limestone bottom from which the water boiled: — in other deep parts of the basin, the depth was about twenty-four feet. The "*Bone-Yard*" (so called from the fact, that several specimens of the bones of the Mastodon have been taken from it), situated two miles below the head-spring, is a *cove* or basin of a similar character. Its *maximum* depth was found to be twenty-six feet.

The most remarkable and interesting phenomenon presented by this Spring, is the *truly extraordinary transparency* of the water; in this respect surpassing any thing which can be imagined. All of the intrinsic beauties which invest it, as well as the wonderful optical properties which popular reports have ascribed to its waters, are directly or indirectly referable to their almost *perfect diaphanity.* On a clear and calm day, after the sun has attained sufficient altitude, — the view from the side of a small boat floating on the surface of the water near the centre of the *head-spring*, is beautiful beyond description, and well calculated to produce a powerful impression upon the imagination. Every feature and configuration of the bottom of this gigantic basin is as *distinctly visible* as if the water was removed, and the atmosphere substituted in its place!

A larger portion of the bottom of this pool is covered with a luxuriant growth of species of water-grass, and gigantic moss-like plants, which attain a height of three or four feet.* The latter are found in the deepest parts of the basin. Without doubt, the development of so vigorous a vegetation at such depths, is owing to the *large amount of solar light* which penetrates these waters. Some parts are devoid of vegetation; these are composed of limestone rock and sand, and present a white appearance. The water boils up from fissures in the limestone; these crevices being filled with sand and comminuted limestone, indicate the ascending currents of water by the local milk-like appearance produced by the agitation of their contents. At these points, my plumb-bob was observed to *bury itself* in the mass of boiling sand.

My observations were made about noon, on the 17th, and again on the 20th of December, 1859. The sunlight illuminated the sides and bottom of this remarkable pool as brilliantly as if nothing obstructed the light. The *shadows* of our little boat, — of our overhanging heads and hats, — of projecting crags and logs, — of the surrounding forest, — and of the vegetation at the bottom, — were distinctly and sharply defined; while the constant waving of the slender and delicate moss-like plants, by means of the currents created by the boiling up of the water, — and the swimming of numerous *fish* above this miniature subaqueous *forest*, — imparted a *living reality* to the scene which never can be forgotten. And if we add to this picture — already sufficiently striking — that objects beneath the surface of the water, when viewed obliquely, *were fringed with the prismatic hues*, — we shall cease to be surprised at the mysterious phenomena with which vivid imaginations have invested this enchanting Spring, as well as at the inaccuracies which have been perpetuated in relation to the wonderful properties of its waters. On a bright day, the beholder seems to be looking down from some

* Subsequent examination of specimens of these plants, indicates that both of them belong to the *Cryptogams*: the *former*, seems to be a species of the genus *Isöetes*, of the Order *Hydropterides*, of the Family of *Lycopodiaceæ*. The *latter*, seems to belong to the genus *Fontinalis*, of the Division *Pleurocàrpi*, of the great Family of *Musci*, or Mosses.

lofty *airy point* on a truly *fairy scene* in the immense basin beneath him: — a scene whose beauty and magical effect are vastly enhanced by the *chromatic tints* with which it is invested!

Popular opinion has ascribed to these waters remarkable *magnifying power.* In confirmation of this, it is commonly reported, that the "New York Herald" *can be read* at the bottom of the deepest parts o the pool. It is almost needless to state, — that the waters *do not possess* this magnifying power, — that it is only the *large capitals,* constituting the *grand heading* of this paper, which can be read at the bottom, — and that the extraordinary *transparency* of the water is abundantly sufficient to account for all analogous facts. A variety of careful experiments were made with the view of testing this point, by securing *printed cards* to a brick which was attached to my fathoming line, and observing at what *depth* the words could be read when viewed *vertically.* Of course, when looked at *obliquely,* the letters were *distorted and colored* by refraction. Numerous *comparative* experiments were likewise executed, in relation to the distances at which the *same cards* could be *read in the air.* The results of these experiments may be announced in a few words, namely: — That, when the letters are of considerable size, — say a quarter of an inch or more in length, — on *a clear and calm day, they could be read at about as great a vertical distance beneath the surface of the water as they could be in the atmosphere!* * Subsequently my young friend Dr. Henry M. Holmes of "Silver-Spring," at my suggestion, repeated several of these experiments, with *identical results.* In some instances, the cards were read by those who were *ignorant* of the words on them. The experiments were made on various sized letters, and at depths varying from six to thirty feet. The comparative experiments in reading the cards in air and water, serve to convey a more distinct idea of the wonderful *diaphanous properties* of the latter, than any verbal description.

Some persons have thought, that there was something mysterious in the fact, that objects beneath the surface of the water, when viewed obliquely, are fringed with *prismatic hues.* It is unnecessary to remind the physicist, that such a phenomenon is a direct physical consequence of the laws of *dispersion of light by refraction.* Observa-

* When the letters are *strongly illuminated,* the reading distance is limited by the *smallness of their images on the retina, and not by the amount of light reaching the eye.*

tion proved, that *white* objects on a *dark* ground were fringed with *blue* at the *top* and *orange* and *red* at the *bottom*; while the color of the fringing was *reversed* for *dark* objects on a *white* ground. This is exactly in accordance with recognized optical principles.* In the present case, the phenomenon is remarkably *striking and conspicuous*, probably, from *two causes: First*, because the extraordinary transparency of the water rendered subaqueous objects *highly luminous; secondly*, because the gigantic evergreens which fringed the pool, cut off most of the *surface reflection*, which would otherwise have impaired the *visual impression* produced by the more feeble refracted and dispersed light proceeding from the objects. The *shadow* of the surrounding forest formed a dark *background*, analogous to the black cloud on which a rainbow is projected.

One of the optical phenomena presented by this Spring, at first sight, seemed somewhat paradoxical: Namely, *that when looking vertically, the depth of the pool appeared to be exaggerated.* This fact was most strikingly and satisfactorily illustrated by the *exaggeration* of the apparent intervals between the bits of white cloth indicating the fathoms on my sounding cord. The fathoms near the surface underwent a somewhat *greater* apparent elongation than those nearer the bottom:—but *all* were exaggerated in length. This phenomenon was observed in all places and under all circumstances; was the *same* whether viewed with *one or both eyes*; and presented the *same* appearance to *all* observers. The *apparent* length of the *upper fathom* was variously estimated at from eight to ten feet. In ordinary cases of considerable obliquity of view, it is a familiar fact, that the water appears to be *shallower* than it really is, in consequence of the seeming *elevation* of the bottom produced by refraction. Hence the foregoing facts in relation to the *apparent exaggeration of depth*, may seem to be inconsistent with recognized optical principles. But a little reflection will show, that when the eye is placed near the surface of the pool, and when we are looking down in a direction approaching the vertical,—the only method of estimating its depth is by means of the apparent intervals between intervening objects:—as for example, the intervals between the branches of a sunken tree and the bottom, or between the fish and the subaqueous

* *Vide*, Herschel's Treatise on Light. *Encyc. Metrop., Article* 429.

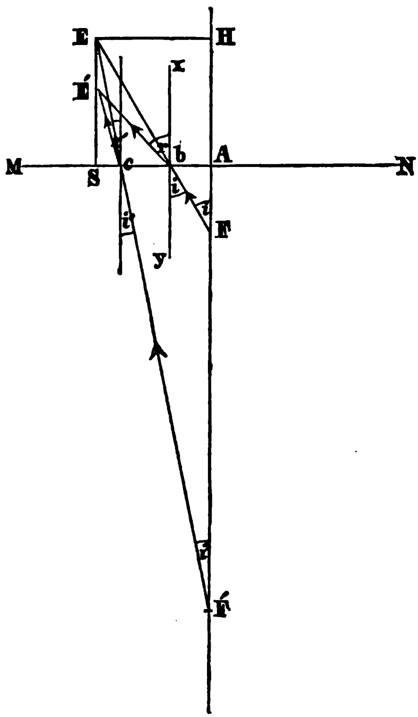
vegetation over which they are swimming, or between the fathom marks on the sounding line. Now, from well-known optical principles, it is obvious, that the apparent distance between two objects thus viewed, depends, *cæteris paribus*, on the *angle* which they subtend at the eye. Thus, for instance, the apparent length of a fathom on the sounding cord, will depend on the magnitude of the angle made by the rays of light proceeding from the *upper* and *lower* bits of cloth which mark the interval. And inasmuch as the rays of light proceeding from the fathom mark which is nearer the surface make a *greater angle of incidence* than those coming from the mark next below, they must undergo a *greater degree of refraction:* — hence the *apparent angle* subtended at the eye by the interval in question (one fathom) is *greater* than if it had been viewed in the air; and, therefore, the length seems to be *exaggerated.*

Moreover, as the *apparent length* of a fathom depends on the angle subtended at the eye, while the degree of refraction is proportional to the *sine of the angle of incidence;* it follows, that when the incidence is *large*, the augmentation of the angle by refraction will be *relatively greater* than when it is *small.* Hence, the uppermost fathom should appear to be *longer* than those below it: — this is precisely in conformity with observation. Our estimate of the *decrease* of these apparent lengths with *increasing depth*, is doubtless vastly exaggerated by the *greater fore-shortening* of the lower fathoms. But as all of them seem to be more or less elongated, and as the *whole depth* is thus, as it were, measured by exaggerated linear units, it must appear to be *greater* than the reality.

The general result to which these optical laws lead is, that to an observer sitting in a boat in the middle of the pool, the bottom near the margin (*if visible at all*, for if the angle of incidence is too *large*, the light from subaqueous objects will be *totally reflected*, and will not emerge from the water), will seem to be *elevated* and the water appear to be *shallower* than it really is; while the bottom near the centre will seem to be *depressed*, and the apparent depth will be exaggerated. In other words, near the *margin*, the depth is measured by the angle made at the eye by the rays proceeding from submerged objects with those coming from the shore line: — this angle being diminished by the refraction of the former, the depth is apparently *diminished.* On the other hand, when looking *directly downwards*, we measure the depth by the angle made at the eye by rays emanating from *upper* and *lower*

submerged objects: — this angle being augmented by the *greater* refraction of the former, the depth is *exaggerated.* These physical principles thus afford a satisfactory explanation of the peculiar *inverted bell-shaped appearance,* which the basin presents to an observer floating near its centre. They likewise explain a fact which strikes the most casual observer; namely, that when the boat is advanced towards an apparently shallow spot situated at some distance, it appears to *grow deeper* as we approach the point in question.

The foregoing is a *general* physical explanation of the phenomenon of exaggeration of depth; but the principles of optics furnish us with the means of submitting it to a *numerical* test, and, consequently, of showing its *adequacy* to account for all of the facts observed. Following out the *graphic method,* the validity of the physical explanation was at first tested by *construction.* While this illustrated in a very satisfactory manner, the accuracy of the deductions which had been drawn, and their entire *adequacy* to explain all of the observed phenomena, — at the same time, it enabled me to throw the question into a more severe mathematical form. Assuming the surface of the water to be a *horizontal plane,* and the sounding-line to be *vertical,* the application of a few elementary principles of geometry and trigonometry will enable us to submit this question to this more rigorous test. In the annexed figure let MAN be the surface of the water, $HAFF'F''$, etc., the sounding-line, E the position of the eye receiving light from the successive fathom-marks $FF'F''$, etc., assuming *no water* to be interposed, and E' the position of the eye receiving the corresponding rays after *refraction* by the water.



Let $ES = HA$ $= h$ (the height of E above the water).
" AF $= d$ (the depth of *first* fathom-mark).
" $FF', F'F''$, etc, $= L$ (the length of the fathoms).
" i, i', i'' etc. $=$ the angles of incidence.
" r, r', r'', etc. $=$ the corresponding angles of refraction.
" n $=$ the index of refraction for water $= 1.336$.

From the law of refraction,

$$\sin r = n \times \sin i, \text{ and } \sin r' = n \times \sin i', \text{ etc.}$$

By geometry the angle subtended at E by $FF' = i - i'$: Also, the angle at E' after refraction $= r - r'$. Then by trigonometry we have

$$\cot i' = \frac{h + d + L}{(h + d) \times \tan i}; \ \cot i'' = \frac{h + d + 2L}{(h + d) \times \tan i}, \text{ etc.}$$

Hence it follows, that when $h, d, L,$ and i are given, $i', i'' i'''$ etc. may be calculated, and consequently r, r', r'', r''' etc. may be found: — hence the angle subtended at E by FF' ($= i - i'$), as well as that subtended at E' ($= r - r'$) by the same fathom-interval become known. For the sake of illustrating this point, let us assume,

$$h = 2 \text{ feet}; \ d = 1 \text{ foot}; \ L = 6 \text{ feet}; \text{ and } i = 30°.$$

Then, by the application of the formula above given, and the geometrical and physical principles already indicated, the following table has been calculated for the foregoing condition of things:

		Angle subtended at E.	Angle subtended at E'.	Ratio of E to E'
$i = 30°$	$r = 41° 54' 46''$	By $FF' = 19° 6' 24''$	By $FF' = 27° 17' 18''$	6:8.57
$i' = 10° 53' 36''$	$r' = 14° 37' 28''$	" $F'F'' = 4° 18' 24''$	" $F'F'' = 5° 48' 34''$	6:8.09
$i'' = 6° 35' 12''$	$r'' = 8° 48' 54''$	" $F''F''' = 1° 52' 18''$	" $F''F''' = 2° 30' 37''$	6:8.05
$i''' = 4° 42' 54''$	$r''' = 6° 18' 17''$	" $F'''F'''' = 1° 2' 41''$	" $F'''F'''' = 1° 23' 56''$	6:8.08

From this it follows that, under the assumed conditions, the *first* fathom-interval is exaggerated in the ratio of 6 to 8.57, whilst the others are elongated in a ratio but slightly greater than six to eight. If the angle i (other conditions being the same) had been taken *larger* the *excess* of the elongation of the *first* fathom-interval would have been *greater*. It thus appears, that all of the fathom-intervals are exaggerated nearly in the ratio of six to eight or three to four: that is, *sen-*

sibly in the ratio of the sines of the angles of incidence and refraction for water. This is only true for *small angles* of incidence, that is, for the *lower* fathoms; for, in that case, the *sines* may be considered proportional to the *angles.* Hence, the *real* depth of this pool being thirty-six feet, the *apparent* depth, assumed to be measured by the exaggerated linear units, would be forty-eight feet.

Strictly speaking, the rays of light emanating from the successive fathom-marks, which, under the assumption of *no* refraction, reach a given point E, *would not all,* after refraction, arrive at the *same* given point E': — so that, in strictness, the rays of light which enter the eye at E', are, *for the lower fathoms,* — those which emerge from the water under *very slightly* different angles of incidence from those given in the table. But it is obvious, that its influence on the deduced *ratio* of the angles subtended at E and E', would be *wholly inappreciable.*

The *rigorous* solution of this problem in physical optics, involves the application of that refined physico-mathematical reasoning, which has not ceased to exercise the ingenuity of some of the greatest geometers, since the period (in 1682), when *Tschirnhausen* first called attention to *caustic curves.* Sir John Herschel shows, that when the refracting surface is *plane,* and the refraction is made from a denser into a rarer medium, as from water into air, "the *diacaustic* curve is the *evolute* of an ellipse, whose major axis is normal to the plane refracting surface; the radiant point being in the lower focus, while the *centre* of the ellipse is at the intersection of the major axis with the refracting surface." * In this case, the radiant is a *fixed point,* and the eye is supposed to view it under all possible angles of emergence. The case which we have under consideration is not materially different. Here the *eye* is *fixed,* and the radiant consists of a *series of equi-distant points descending vertically.* In fact, MM. Engel and Schellbach, in their admirable *Darstellendende Optik,* have presented us with beautiful *graphic* representations of the exaggeration and distortion which submerged objects undergo when viewed by the eye placed in various positions above the plane refracting medium. †

* *Vide,* Herschel's Treatise on Light. *Encyc. Metrop. Article* 238, *Fig.* 39.

† *Vide,* Darstellendende Optik, von F. Engel und K. Schellbach. — *Halle,* 1856. *Taf.* 1. *Fig.* 3 et 4. Under such conditions, submerged objects not only appear to be exaggerated in length, but they seem to be more or less *curved* in the common plane of incidence and refraction; the centre of concavity being towards the observer.

Thus, it has been shown, that all of the beautiful optical phenomena presented by the Silver-Spring are referable to recognized physical principles, and that all of the so-called mysteries of its waters vanish under the scrutiny of exact science. It only remains to indicate the *causes* which produce the extraordinary *transparency* of the water, upon which, as has been shown, the entire group of phenomena is dependent. It may be remarked, that these diaphanous properties are *perennial*: — they are not in the slightest degree impaired by season, — by rain or drought. The comparatively slight fluctuations in the level of the water in the pool, to which allusion has been made, produced by the rainy season, are *not* (according to the uniform testimony of the residents), accompanied by any turbidity of its waters. At first sight, it may seem paradoxical that, in a country where semi-tropical summer rains occur, the waters of this stream should not be rendered turbid by the surface drainage. But the whole mystery vanishes when we consider the peculiar character of the drainage of this section of Florida. Although the surface of the country is quite undulating or *rolling*, — the summits of many of the hills being thirty or forty feet above the adjacent depressions; yet, *there is no surface drainage*; there is not a *brook*, *rivulet*, *branch*, or *swamp* to be found in this part of the State. The whole drainage is *subterranean*: even the water which falls near the banks of the Silver-Spring passes out by underground channels. There is not the slightest doubt, but that all of the rain water which falls on a large hydrographic basin passes down by subterraneous channels, and boils up and finds an outlet to the St. John's river, by means of the Silver-Spring and the smaller tributary springs which occur in the *coves* along the margin of the stream. The whole surface of the country in the vicinity of Ocala, and probably over the area of a circle of fifteen miles radius, whose centre is the Silver-spring, is thickly dotted with *lime-sinks*, which are the points at which the surface water finds entrance to the subterranean passages. New *sinks* are constantly occurring at the present time. The beautiful miniature lakes, whose crystal waters are so much admired, which occur in this portion of Florida, are, doubtless, nothing more than extensive *lime-sinks* of more ancient date.

Under this aspect of the subject, it is obvious that all the water which falls on this hydrographic basin boils up in the Silver-Spring after having been *strained*, *filtered*, and *decolorized* in its passage through

beds of sand and tortuous underground channels. It thus comes out not only entirely free from all mechanically suspended materials, but completely destitute of every trace of *organic coloring matter*. According to the *barge-men*, there is a striking *contrast* between the color and transparency of the waters of the Silver-Spring and the Ochlawaha at their junction. The latter river drains a country whose drainage is *not entirely subterraneous*. In addition to the above-mentioned conditions, which persistently secure the waters of this spring from the admixture of insoluble materials as well as from the discoloration of organic matters, it seems highly *probable, that the minute quantity of lime which they hold in solution may exercise some influence in augmenting their transparency; for they appear to be more diaphanous than absolutely pure water*. There is nothing *à priori* improbable in the idea, that the *optical* as well as the other physical properties of the liquid, are altered by the materials held in solution. This is an interesting physico-chemical question, which demands experimental investigation. It is proper to add, that the waters of the Silver-Spring *are not* charged with more than the ordinary amount of carbonic acid; they deposit *no carbonate of lime*; so that the amount of lime held in solution must be comparatively small.

Doubtless there are many other springs to be found in the State of Florida, whose waters possess the same optical properties as those of the Silver-Spring; although, perhaps, their transparency may be less perfect. The "Suwanee Spring" is said to exhibit analogous phenomena; and the famous fountain situated ten miles from Tallahassee, called Wachulla or Wakulla, is represented as "an immense limestone basin, as yet unfathomed in the centre, with waters as transparent as crystal." Inasmuch as I have not examined these springs, I am unable to say, how far the optical phenomena which they present, may be *identical* with those exhibited by that which is the subject of this paper.

As in some measure related to the peculiar system of subterranean drainage above indicated, it may not be deemed inappropriate to conclude this communication with a few general remarks in relation to the physical causes which have produced the several qualities of surface soil, which are found in the neighborhood of Ocala and the Silver-Spring. The whole of this portion of the peninsula appears to have been originally composed of a mixture of sand and shell-limestone,

probably of the Eocene period. The lime-rock comes to the surface, almost everywhere: in some cases, it is composed of nearly pure carbonate of lime; in others, silicification, to a greater or less extent, has taken place by the displacement of the lime by silex. But in all cases where its structure can be made out, it consists of a mass of conglomerated shells. The *three grades of fertility* at present existing in the soil of this portion of the State, appear to be owing to the *greater or less facility with which the lime has been removed from it by aqueous agencies.* In the fertile and densely wooded Hammock lands, large quantities of soft carbonate of lime may be found at or near the surface. In the Mulatto pine lands, which are now extensively cultivated in cotton and Indian corn, the amount of surface carbonate is less abundant, a considerable portion of it having been either silicified or removed from the soil. While in the sterile sandy pine lands, no lime is to be found; the whole of the rock having disappeared, excepting that which has undergone silicification. In the Hammocks, an impervious *substratum of clay* has prevented the lime from being carried off by the percolation of meteoric waters; in the Mulatto lands (so called because there is a subsoil of *yellow* clay), the substratum is less impervious, so that a large portion of the lime has been removed; while in the pine barrens, in consequence of the absence of a clay subsoil, the *whole of the surface lime* has been carried off by subterranean drainage, leaving no surface rocks excepting those which are silicified. According to this view, the light pine lands which now produce cotton with so little labor, are in the *transition stage* to the pine barrens, and cannot be expected to retain their fertility for any great length of time, unless lime is restored to them by the cultivator. The heavily timbered Hammocks require a greater outlay to bring them under cultivation, but they constitute the most valuable and enduring lands in this section of the State. Unfortunately they embrace but a comparatively limited area, when contrasted with the space occupied by the pine lands. The outlines of the Hammocks, as indicated by the dense growth of gigantic evergreens, are singularly and sharply defined, either dotting or intersecting the desolate pine-barrens; sometimes forming narrow sinuous verdant bands extending ten or fifteen miles, which, at a distance, remind one of extensive swamps, or the bottom lands bordering a stream.

2. On the Sudden Disappearance of the Ice of our Northern Lakes in the Spring. By Gen. J. G. Totten, Chief Engineer of the U. S. Army.

Some forty years ago, being at Plattsburg, New York, on the margin of Lake Champlain, and not far from the widest part of the lake, I had a favorable opportunity for studying the phenomenon of the sudden disappearance of the great body of the ice covering that lake; a body of very many square miles in extent, and not less than one foot in thickness.

This striking phenomenon has often given rise to wild speculation and conjecture in the unscientific world. It was the subject of discussion in this Association; and it is under the impression, perhaps erroneous, that full information was not then and has not been since presented, that I now venture to produce the following substance of my observations, though made chiefly at that distant day.

At the close of a day in April, I think, the whole surface of Lake Champlain, with the exception of a very few "air holes" or unfrozen portions of at most a few acres each, and a strip of water next the shores, was one great expanse of ice, of a thickness not less than twelve inches; and apparently, looking merely at the surface, as solid as ever.

During the following night, there arose a strong wind from the southward, blowing, therefore, nearly lengthwise of the lake; and when I looked out the following morning, not a particle of ice was to be seen; but instead thereof, a lively play of water sparkling with "white caps." There was, as determined by immediate and close examination, absolutely no ice upon the water nor in the water; not a fragment, large or small. Upon the lee shore of a bay close at hand, there was, however, a fringe of broken ice that had been washed up by the waves; and in the condition of these few remains of the night's work was to be found, it seemed to me, a satisfactory explanation of a change certainly very surprising from its suddenness and completeness; and deemed indeed even by high authority in philosophy, so much to partake of the marvellous as to require a higher solution than philosophy was able, consistently to supply.

I venture, in offering this mite to the collections of the Association, to give the explanation then suggested by my examinations; because, as intimated before, I am not aware that such particulars as I have to describe, have been connectedly given, although they must have been often exhibited to individual observation, and as often, one would think, have led to an explanation simple, satisfactory and clearly within the domain of the consistent philosophy that nature loves.

The fringe of broken ice was found to consist wholly of prismatic fragments; all of which, excepting a few broken transversely, were of uniform length, namely a length exactly equal to the thickness of the mass of ice of which they had been portions.*

The sides of these fragments were irregular as to number and form; the breadth or thickness varying sometimes in the same prism from three quarters of an inch to an inch and a half — perhaps a little more or less; but notwithstanding such variations, there was a general agreement as to shape and size; and the general result in all was a decidedly prismatic form. There were, moreover, sometimes to be seen upon their irregular sides, portions of some length that were probably true crystalline faces. Excepting a small portion at one end of each that was evidently made up of half-melted and refrozen snow, they were very transparent, with few air-bubbles, and as sonorous, nearly, as similar prisms of glass.

Examinations then and afterward of floating freshwater ice (which alone I have observed), have shown that the natural effect of the advancing year is gradually to transform ice, solid and apparently homogeneous, into an aggregation of these irregular prismatic crystals, standing in vertical juxtaposition, having few surfaces of contact, but touch-

* The following description and remarks, so far as relates to details, belongs to the particular case then, so much to my astonishment and surprise, presented to notice. I have observed since, that the circumstances under which bodies of freshwater ice are formed, are not always favorable to so clear an exhibition of the law of structure. The vertical arrangement of elongated solid pieces, although sometimes quite irregular as to shape and dimensions, and interrupted, and lapping in their length, is however, I believe, always to be seen in blocks of ice in which solution is somewhat advanced, and to be detected by cleavage, unless, indeed, the process of congelation has been disturbed by forces too great for an observance of the law of crystallization. Such deviations do not however, it is thought, touch the general conclusions to which our case seems to lead us.

ing rather at points and on edges, and kept in place at last, merely by want of room to fall asunder. Until this change has somewhat advanced, the cohesive strength of ice of considerable thickness is still adequate to sustain the weight and shock of the travel it had borne during the winter; but becoming less and less coherent, by the growing isolation of the prisms, or more and more "rotten," as the phrase is, though retaining nearly all its thickness, the ice will at last scarcely support a small weight, though bearing upon a large surface — the foot of a man easily breaking through, and very slight resistance being made to the point of a cane.

Before describing the peculiar preliminary process by which ice is brought to this condition, it may be well to follow out the steps by which the striking phenomenon of the sudden disappearance, by melting, of vast fields of thick ice is accomplished. The final process of dissolution will vary somewhat with circumstances; but in all cases where the ice has been, so to speak, duly prepared, nothing is wanting to a quick disappearance, but a disruption of the few remaining surfaces of contact in the prismatic assemblage. If this be not abruptly effected by undulations in the field of ice, solution will continue to erode the sides of the constituent prisms, until, being no longer in contact, or adequately sustained laterally, each will drop into the position in the water below, required by the place of its own centre of gravity — that is to say, it will lie upon its side, exposing large surfaces to the action of the warm water. It is easy to see that this will occur, if not simultaneously, with all the prisms in rapid succession.

But the effects, in the instance that first drew my attention, were the results of violence; causing the greater surprise, by suddenly bringing about what, according to the calm process above indicated, would have been postponed for many days.

The condition of the ice on Lake Champlain on the day in April before mentioned, being a mere aggregation of vertical prismatic crystals, cohering only at points and along edges and narrow surfaces, as shown next morning by fragments on the shore, it could oppose little resistance to waves raised by the wind of the following night. These acting first upon the edges of the "air-holes," and open spaces between the ice and the shore, caused slight undulations there in the ice itself, and the consequent falling apart of the feebly cohering prisms; so that, the water surfaces being thereby enlarged, a short time only was neces-

sary for the waves, increasing in altitude and force with the enlarging water surfaces, to send their undulations far before them under the yielding ice. The prisms falling upon their sides, all more or less immersed, affording now large surfaces to the solvent action of water above the melting temperature, and stirred about by the waves, were quickly dissolved. It is not easy to say in how short a time, under such circumstances, the great transformation would be wrought, but there ought to be no surprise that all was accomplished in the eight or ten hours of a spring night.

The preliminary process, before alluded to, of the conversion of masses of solid ice into an aggregation of vertical prisms by partial solution, must be dependent on the fact that the law of crystallization in that substance yields prisms with vertical axes. That this is the law is indicated by cleavage as well as by solution; for while this is easy and free in planes perpendicular to the upper surface, it is said, truly I believe, not to be attainable in directions oblique or parallel thereto. Beyond this general fact of a vertical arrangement of prisms, it is not necessary to go for elucidation of our subject, even if I could give minute specifications as to the crystallization of ice. I am not aware, indeed, that this question in crystallography, interesting as it might prove, has been very thoroughly investigated; but however that may be, we have demonstrated to us by the natural process of solution, that ice formed as in the case before us, however solid and homogeneous in appearance, contains a hidden array of crystalline prisms. So much is certain; and this, for the present, is enough on that point. May we not further assume, that in the process of arrangement about the axes of these prisms, as they are projected downwards into the freezing water, the particles of water, in obeying the law of crystallization, crowd out, radially, the portions of air that would otherwise interfere with their just disposition as ice; and that, at last, this air, by accumulation in spaces between the prisms, suffices to prevent further obedience to the symmetrical principle, causing, in these spaces, a confused and porous crystallization peculiarly favorable to the action of a solvent? Whether this be the precise cause or not, a condition favorable to dissolution certainly exists in the irregular spaces between the prisms, as we see by the particulars before given.

The process of Daniel for bringing to view the innate crystallization of apparently amorphous masses — namely, submitting them to the

action of a solution of the same substance, so nearly saturated as to exercise solvent power only when the solidification is imperfect — seems to afford a close analogy to that followed by nature, in preparing ice for quick dissolution.

The natural action seems to be this. The early rains of spring throw upon the surface, and, by the tributaries, pour under the fields of ice, frequent supplies of water, at a temperature melting even at first, and rising with the progress of the year. This warm underlying water, acting chiefly on the porous spaces between the prisms, dissolves them out to the full depth to which the ice is immersed, and perhaps still further, by capillary action. At the same time, the spongy ice, formed upon the upper surface by melted and refrozen snow, affords, by melting, warm water, to affect similarly the porous spaces between the tops of the prisms.

In this way, during the considerable period intervening between the first spring rains and the final breaking up of the lake, the solid ice is transformed into the condition necessary to a sudden dissolution.

We may assume, indeed, that the solvent action begins on the lower surface, about the time the accretion, by further freezing, ceases; that it proceeds very slowly, so long as the temperature of the water remains below that of the greatest density, and of course that it goes on more rapidly as the water is lifted above that temperature by the growing warmth of spring.

I regret that I did not take the temperature of the water in the morning after the disappearance of the ice; but on this point I may add to what is said above, that the spring was then well forward; all, or nearly all, the snow had melted from the fields; the early rains and melted snows had for some time been raising the lake, which was then nearly at its greatest height. It was this rise in the lake that had spread a margin of water *that did not freeze* between the great field of ice and the shore. The inference from all the circumstances, that the temperature of the water at the time of disruption, and for some time previous, was not only above the melting point, but also above that of maximum density, seems to me unavoidable.

I may here be permitted to mention another matter connected with fields of lake-ice that has excited some wonder; namely, the movement towards the shore of boulders, sometimes quite large. The process which must have occurred to intelligent observers, and has prob-

ably been heretofore explained, seems to be this: after the rising of the water has supplied an unfrozen margin, a strong wind will sometimes cause the whole field to move until its edge meets adequate resistance upon the shore, all boulders encountered in the way, being pushed before it, into an array upon the shore that accurately marks the extent of the invasion. These lines of boulders are to be seen in many places, registering accurately, not the work of the preceding year, but the greatest effort of any previous year.

The circumstances of some deep-lying boulders may be such that they are rarely embraced, acted on, or moved; and such may long, by fits, continue to be erratic, though finally to join the general shore parade.

The force of these moving fields is very great, even when the decomposing process is much advanced. I have seen a timber wharf, which was about thirty feet square, ten or fourteen feet high, and filled solidly with earth and stones, shoved along the bottom about thirty feet, by a single continuous push of a great field of ice just ready to be resolved into its prismatic elements. The motion was very slow; only to be seen, indeed, by close observation; while the ice was broken at the edge of contact into innumerable fragments, piling themselves, with a tinkling sound, high upon the wharf and following ice.

A simple and effectual guard against this danger to wharf or pier has been found to be, the giving to the exposed face a certain talus (about one of base to two of height, I think), which turns the ice upwards to the top of the structure, where its fragments accumulate, sometimes to a considerable height. This easy diversion of so great a force is due, of course, to the peculiar crystalline structure of the ice, the degree to which it has been decomposed, and the consequent brittleness against a transverse strain. Should there be an unfrozen margin to permit this motion of large fields of ice, before the solution of continuity in the crystalline arrangement, nothing but the solid earth could stand before it.

These remarks have extended further than I intended; and I fear much beyond what was required by the state of knowledge on the subject. But I venture, nevertheless, in reference to the first portion of these remarks, one further observation, namely, that nature seems to have especially provided, in the structure of these wintry coverings of water surfaces, for their prompt removal, when their existence would retard the advancing year.

3. Investigation of the Problem regarding the Existence of a Lunar Tidal Wave on the great Freshwater Lakes of North America. By Brevet Lieut.-Colonel James D. Graham, Major of Topographical Engineers, U. S. Army.

Much has been written, at various periods, on the fluctuations in the elevation of the surface waters of the great freshwater lakes of North America.

Valuable and interesting memoirs have appeared from time to time in the American Journal of Science and Arts, published monthly at New Haven, Connecticut, within the last thirty years, on this subject, written by the late Brevet Brigadier-General Henry Whiting, of the U. S. Army, when a captain, by Major Lachlan, Charles Whittlesey, Esq., and others. The observations contained in their memoirs have, however, been directed chiefly to investigations of the extent of the secular and annual variations in elevation of the surfaces of these lakes.

The learned Jesuit fathers of the time of Marquette, a period near two centuries ago, and at later periods the Baron de la Hontan, Charlevois, Carver, and others, noticed in their writings the changes of elevation, and some peculiar fluctuations which take place on these inland seas. In the speculations indulged in by some of these writers a slight lunar tide is sometimes suspected, then again such an influence on the swelling and receding waters is doubted, and their disturbance is attributed to the varying courses and forces of the winds.

But we have nowhere seen that any systematic course of observation was ever instituted and carried on by these early explorers, or by any of their successors who have mentioned the subject, giving the tidal readings at small enough intervals of time apart, and of long enough duration, to develop the problem of a diurnal lunar tidal wave on these lakes. The general idea has undoubtedly been that no such lunar influence was here perceptible.

In April, 1854, I was stationed at Chicago by the orders of the Government, and charged with the direction of the harbor improvements on Lake Michigan. In the latter part of August of that year, I caused to be erected at the east or lakeward extremity of the North harbor pier, a permanent tide-gauge for the purpose of making daily observa-

tions of the relative heights and fluctuations of the surface of this lake. The position thus chosen for the observations projects into the lake, entirely beyond the mouth of the Chicago River, and altogether out of the reach of any influence from the river current, upon the fluctuations of the tide-gauge. It was the fluctuations of the lake surface alone, that could affect the readings of the tide-gauge.

On the first day of September, 1854, a course of observations was commenced on this tide-gauge, and continued at least once a day, until the 31st day of December, inclusive, 1858. During each of the first three winters a portion of the daily observations was lost, owing to the tide-gauge being frozen fast in its box, but they constituted only a small number in proportion to that embraced in the series. During the subsequent winters artificial means were resorted to, to prevent this freezing. These observations will be found printed, in my public reports, in the following Congressional documents, namely, Senate (executive) Doc. No. 16 of the 34th Congress, 3d session, at pp. 85 to 98, and 321 to 328; Senate (executive) Doc. No. 42 of the 35th Congress, 1st session, pp. 24 to 35; Vol. 2, Part 2 (marked on the back "Part 3") of documents accompanying the President's Message to the 35th Congress, at the commencement of its 2d session, pp. 1105 and 1106.

These observations were instituted chiefly for the purpose of ascertaining with accuracy the amount of the annual and also of the secular variation in the elevation of the lake surface, with a view to regulating the heights of break-waters and piers to be erected for the protection of vessels, and for improving the lake harbors.

A close inspection of these observations, and a comparison of the observed elevation of the lake surface at the periods when the moon was on the meridian and in the horizon, showed clearly that there was a difference at these two periods amounting, in the average of all the tides, to as much as fifteen hundredths (.15) of a foot, and in the average of the spring tides or those which occurred at the conjunction and opposition of the moon with the sun, to at least as much as two tenths (.2) of a foot.

This result was announced in my annual report to the Topographical Bureau of the War Department on the 15th of November, 1858, and also before the Chicago Historical Society at its annual meeting, on the 30th of that month. See pp. 1107 and 1108 of Vol. 2, Part 2, of Docs. sent with the President's message to the 35th Congress at the

commencement of its 2d session, December, 1858; also, p. 39, Vol. 3 of the Historical Magazine, published monthly at New York, by C. B. Richardson.

For the purpose of a more complete investigation of this problem, we instituted a new series of observations upon the tide-gauge already alluded to. It was commenced on the 1st day of January, 1859, and continued to the 1st of July, inclusive, of that year. It embraces nine thousand one hundred and eighty-four (9,184) observations made at consecutive intervals of half an hour apart, as a general rule, and sometimes at intervals of fifteen (15) minutes apart, and continued uninterruptedly, both day and night, with the exception of two hundred and thirty-two (232) observations which were lost, chiefly in the stormy portions of the months of February and March. But for these unavoidable losses, the series would have consisted of 9,416 observations.

We have prepared a paper showing all these observations in full detail, with remarks upon the winds, the weather, &c., for future publication, with the view of exhibiting every vicissitude, favorable and unfavorable, which attended the series. The half hour (and in four instances the quarter hour), coördinates of elevation of the surface of Lake Michigan, reckoned from the period (before and after) of the moon's meridian transit, resulting from a mean of all the observations appertaining to each coördinate, and expressed in decimals (thousandths) of a United States foot, were found to be as follows, namely:—

TABLE I. — SHOWING THE HALF-HOURLY (AND, AT TWO PERIODS, THE QUARTER-HOURLY), COÖRDINATES OF ALTITUDE OF THE AVERAGE SEMI DIURNAL LUNAR TIDAL WAVE AT CHICAGO, ON LAKE MICHIGAN, DERIVED FROM THE WHOLE 9,184 OBSERVATIONS MADE FROM JANUARY 1, TO JULY 1, 1859.

	Mean Solar interval of time before or after the Moon's meridian passage.	Observed elevation of the lake surface, in decimals of a foot.	
	h. m.	Ft. Decimals.	
Before the Moon's meridian passage.	5 35	0.000	Lunar low water.
	5 30	0.004	
	5 00	0.008	
	4 30	0.016	
	4 00	0.030	
	3 30	0.040	
	3 00	0.053	
	2 30	0.078	
	2 00	0.087	
	1 30	0.089	
	1 00	0.115	
	0 30	0.130	
Moon in meridian. . .	0 00	0.140	Moon in the meridian.
After the Moon's meridian passage.	0 30	0.146	Lunar high-water.
	0 45	0.143	
	1 00	0.134	
	1 15	0.134	
	1 30	0.130	
	2 00	0.116	
	2 30	0.112	
	3 00	0.082	
	3 30	0.066	
	4 00	0.048	
	4 30	0.040	
	5 00	0.032	
	5 30	0.024	
	6 00	0.030	Slightly discrepant owing to a preponderance of unfavorable winds at this particular period.
	6 30	0.012	
	6 50	0.000	Lunar low-water.

The accompanying profile, marked Fig. 1, shows the character of a semi-diurnal tidal wave passing through our tide-gauge, as deduced from the whole series of observations made, and projected from the foregoing table. They both show an average difference between the elevation of the surface of this lake, at lunar low and lunar high water; embracing every vicissitude, whether favorable or unfavorable, of winds, weather, &c., during the six months' observations, of one hun-

dred and forty-six thousandths (.146) of a foot. They also show that the average time of lunar high-water at Chicago is thirty minutes after the time of the moon's meridian transit.

On examining with scrutiny all the observations embraced in the series, we find one hundred and eighty-nine (189) which we think may be fairly rejected, because influenced in an extraordinary degree by unfavorable winds. This would reduce the number of observations remaining in the series to 8,995. From these, the half-hour and quarter-hour coördinates of altitude are deduced as follows, namely: —

TABLE II. — SHOWING THE HALF-HOURLY (AND AT TWO PERIODS THE QUARTER-HOURLY) COÖRDINATES OF ALTITUDE OF THE AVERAGE SEMI-DIURNAL LUNAR TIDAL WAVE AT CHICAGO, ON LAKE MICHIGAN, AS DERIVED FROM 8,995 OUT OF THE 9,184 OBSERVATIONS MADE BETWEEN JANUARY 1 AND JULY 1, 1859.

	Mean Solar interval of time before or after the Moon's meridian passage.	Observed elevation of the lake surface in decimals of a foot.	
	h. m.	Ft. Decimals.	
Before the Moon's meridian passage.	5 35	0.000	Lunar low-water.
	5 30	0.005	
	5 00	0.004	
	4 30	0.013	
	4 00	0.030	
	3 30	0.041	
	3 00	0.054	
	2 30	0.078	
	2 00	0.090	
	1 30	0.098	
	1 00	0.108	
	0 30	0.127	
	0 00	0.148	Moon in the meridian.
After the Moon's meridian passage.	0 30	0.153	Lunar high-water.
	0 45	0.148	
	1 00	0.137	
	1 15	0.134	
	1 30	0.132	
	2 00	0.113	
	2 30	0.107	
	3 00	0.084	
	3 30	0.056	
	4 00	0.040	
	4 30	0.031	
	5 00	0.025	
	5 30	0.023	
	6 00	0.024	
	6 30	0.010	
	6 50	0.000	Lunar low-water.

The accompanying profile, marked Fig. 2, shows the character of the semi-diurnal wave projected from this modified general result. It shows a difference of elevation, between the high and low-water coördinates, of one hundred and fifty-three thousandths (.153) of a foot, and it still indicates thirty minutes after the time of the moon's meridian passage, or southing, as the average period of lunar high-water.

From one day before, to two days after the periods of the moon's conjunction with, and opposition to the sun, the observations upon the tide-gauge were made continuously, both day and night, at regular intervals of fifteen (15) minutes of time apart. This was done with an especial view to obtain, as near as possible, the time of lunar high-water compared with that of the moon's meridian passage or southing, at the period of spring tides; and also with a desire to ascertain, with all practicable accuracy, the measure of the quarter-hourly coördinates of altitude of the lake surface when under the maximum influence produced by the combined attraction of the sun and moon, acting in the same or in nearly the same direction.

For this object a separate tabulation was made, of all the quarter-hourly observations which occurred from about twelve (12) hours before, to twenty-four (24) hours after the periods of the conjunction and opposition of the sun and moon, from the new moon of January 3, to the new moon of June 1,* inclusive, excepting, however, the observations which occurred about the period of the new moon of April 3, a portion of those which occurred at the period of the full moon of March, and portions of those which occurred at the periods of the new and full moon of May. Those which were excluded were too violently affected by the stormy and unfavorable winds which then prevailed, to make it proper to admit them in making up the mean altitudes of the coördinates for the spring tides. The number of unobjectionable observations that were collated for this purpose amounted, however, to 1,200, so that each quarter-hour coördinate of altitude for the spring tides is deduced from a mean of twenty-four (24) actual observations, as shown in the following Table No. 3:

* Unfortunately the winds were so boisterous, and caused so great perturbations of the lake surface, at the periods of full moon of June 15, and the new moon of June 30, that we were obliged to reject the observations made at those periods, in making up the coördinates of altitude for the spring tides. J. D. G.

TABLE III. — SHOWING THE QUARTER-HOURLY COÖRDINATES OF ALTITUDE OF THE AVERAGE SEMI-DIURNAL LUNAR SPRING TIDAL WAVE AT CHICAGO, ON LAKE MICHIGAN, AS DERIVED FROM 1,200 OBSERVATIONS MADE BETWEEN JANUARY 3 AND JUNE 2, 1859, AT AND NEAR THE SEVERAL PERIODS OF NEW AND FULL MOON.

	Mean Solar interval of time before or after the Moon's meridian passage.	Observed elevation of the lake surface, in decimals of a foot.	
	h m.	Ft. Decimals.	
Before the Moon's meridian passage.	6 00	0.000	Low-water of lunar spring tide.
	5 45	0.006	
	5 30	0.014	
	5 15	0.029	
	5 00	0.035	
	4 45	0.042	
	4 30	0.049	
	4 15	0.057	
	4 00	0.079	
	3 45	0.081	
	3 30	0.089	
	3 15	0.091	
	3 00	0.101	
	2 45	0.121	
	2 30	0.129	
	2 15	0.145	
	2 00	0.153	
	1 45	0.169	
	1 30	0.178	
	1 15	0.187	
	1 00	0.195	
	0 45	0.216	
	0 30	0.225	
	0 15	0.226	
	0 00	0.233	Moon in the meridian.

BLE III. — *continued.* SHOWING THE QUARTER-HOURLY COÖRDINATES OF ALTITUDE OF THE AVERAGE SEMI-DIURNAL LUNAR SPRING TIDAL WAVE AT CHICAGO, ON LAKE MICHIGAN, AS DERIVED FROM 1,200 OBSERVATIONS MADE BETWEEN JANUARY 3 AND JUNE 2 1859, AT AND NEAR THE SEVERAL PERIODS OF NEW AND FULL MOON

	Mean Solar interval of time before or after the Moon's meridian passage.	Observed elevation of the lake surface, in decimals of a foot.	
	h. m.	Ft. Decimals.	
After the Moon's meridian passage.	0 15	0.248	
	0 30	0.254	High-water of Lunar spring tide.
	0 45	0.241	
	1 00	0.229	
	1 15	0.226	
	1 30	0.221	
	1 45	0.221	
	2 00	0.201	
	2 15	0.179	
	2 30	0.161	
	2 45	0.140	
	3 00	0.120	
	3 15	0.112	
	3 30	0.103	
	3 45	0.093	
	4 00	0.072	
	4 15	0.066	
	4 30	0.072	Slightly discrepant owing to a preponderance of unfavorable winds at this particular period.
	4 45	0.067	
	5 00	0.059	
	5 15	0.046	
	5 30	0.040	
	5 45	0.042	Slightly discrepant owing to the same cause.
	6 00	0.050	
	6 15	0.027	
	6 28	0.000	Low-water of Lunar spring tide.

The lunar semi-diurnal spring tide, averaged in the above Table III is graphically shown in the accompanying profile marked Fig. 3. The coördinates of altitude are given for every fifteen minutes of elapsed time, from low to high-water, and thence to the succeeding low-water. The difference of elevation of the lake surface, between the periods of lunar low and lunar high-water at the mean spring tides is here shown to be two hundred and fifty-four thousandths (.254) of a foot, and the time of high-water at the full and change of the moon is shown to be thirty (30) minutes after the time of the moon's meridian transit.

We, therefore, in accordance with custom in like cases, indicate as the *establishment* for the port of Chicago,

$$\tfrac{1}{4}\text{ Foot, } \overset{\text{h.}}{0}\ \overset{\text{m.}}{30}.$$

Although this knowledge may be of but small practical advantage to navigators, yet it may serve as a memorandum of a physical phenomenon whose existence has generally heretofore been either denied or doubted.

We think it probable that, if the effect of unfavorable winds and all other extraneous forces which produce irregular oscillations in the elevation of the lake surface could be fully eliminated, a semi-diurnal lunar spring tide would be shown of as much as one third of a foot for the periods of highest tides.

The time of low-water and the relative times of duration of the flood and ebb tides are given only approximately. The extreme rise of the tide being so little, the precise time of the change from ebb to flood and hence the duration of the flow of each, can only be accurately determined by numerous observations at short intervals, say three to five minutes of time apart, from about an hour before to an hour after the actual time of low-water.

In conclusion, we offer the above observations as solving the problem in question, and as proving the existence of a semi-diurnal lunar tidal wave on Lake Michigan, and consequently on the other great freshwater lakes of North America, whose coördinate of altitude is, at its summit, as much as .15 to .25 ($\frac{15}{100}$ to $\frac{25}{100}$) of a foot, United States measure.

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4. ABSTRACT OF THE PRINCIPAL RESULTS OF THE OBSERVATIONS OF THE TIDES AT VAN RENSSELAER HARBOR MADE BY THE SECOND GRINNELL EXPEDITION UNDER COMMAND OF DR. E. K. KANE, U. S. N., DURING 1853, 1854, and 1855, from a reduction and discussion by CHAS. A. SCHOTT, Assistant Coast Survey.

[Presented by Prof. A. Bache, Superintendent Coast Survey.]

IT is proposed to present the principal results of the observations and discussion of the tides at Van Rensselaer Harbor. This paper forms the sixth and last of the series prepared by me for publication.*

Occasional tidal observations were made after passing Smith's Straits where, owing to the peculiar navigation through the narrow openings between the coast and the bay ice, the vessel was much exposed to the tidal action, frequently grounding at low-water and otherwise, by taking advantage of high tides; slowly advancing to her winter-quarters. The bay, near the head of which the Advance was laid up, is freely exposed to the north and northwest (true), the indentation of the shore line is about five miles; some rocky islands are situated within the bay. Shortly after the vessel entered the harbor, a tide-staff was arranged, and a series of tidal observations were commenced on Sept. 11, 1853, and continued with occasional interruptions (partly owing to defects in the pulley-gauge afterwards rigged up, and partly owing to other unavoidable accidents), till the 24th of January, 1855, on which date the regular log-book appears to have been discontinued.

The several series of the observations during this time are of very unequal value as will appear in the detailed examination and discussion of the results. The difficulties to be overcome in the attempt to secure a reliable set of observations were considerable, those of a physical nature being the greatest. The observations with the staff or sounding-line are subject to irregularities from a slow movement of the vessel, which, though imbedded in ice, is yet not stationary during the greater part of the year; these observations may also be affected by

* For the complete papers, see Smithsonian Contribution to Knowledge, Volume XII.

the softness of the bottom; the observations by means of a pulley tide-gauge may be defective on account of a slow drift of the vessel and motion of the ice-field, also in consequence of a lengthening or shortening of the rope, or it may be in consequence of the slipping of the rope or chain on the circumference of the wheel. This latter defect or one similar in its nature has been the source of much annoyance, and requiring the application of corrections to the readings in order to refer all observations to the same zero of the scale. There is another defect to which pulley-gauges are subject, it is the gradual rise of the vessel, in consequence of the consumption of provisions and fuel.

The pulley-gauge is described by Dr. Kane as follows: "Our tide register was on board the vessel, a simple pulley-gauge, managed with a wheel and dependent on her rise and fall for its rotation; one end of the cord represented a fixed point by being anchored to the bottom, the free end with an attached weight rose and fell with the brig and recorded its motion on the grooved circumference of a wheel. This method was liable to objections, but it was connected by daily soundings. The movement of our vessel partook of those of the floe in which she was imbedded."

In order to ascertain the nature of the tides as well as the degree of reliability of the different observations, the readings were roughly plotted for a first examination, the following series were found suitable for discussion: —

Series I. from Oct. 10, 1853, to Dec. 28, 1853. This series with the exception of three days is complete; the observations in the latter part of December appear to be of a less reliable character. The hourly readings are superseded by half-hourly readings on Nov. 8th, and continue so to the end of the series. After Nov. 28, corrective soundings were taken at noon each day. In order to make use of these soundings, the mean depths of the water at the anchorage was deduced from them, as follows: —

December, 1853,	mean	depth	from	31	soundings,	43.8 feet.
January, 1854,	"	"	"	21	"	44.9 "
February, "	"	"	"	17	"	44.3 "
March, "	"	"	"	19	"	43.3 "
April, "	"	"	"	20	"	41.8 "
May, "	"	"	"	9	"	43.5 "
Mean depths of the water in winter of 1853–54,						43.6 "

For series I. (and all subsequent series), the reading 7.0 was adopted to express the plane of reference; the zero of the scale was therefore at an elevation of 36.6 feet from the bottom. The readings of the pulley-gauge are expressed in feet, as I have been informed by Mr. Sontag.

Series II. from January 28, 1854, to April 7, 1854. The double half-hourly readings of the pulley-gauge are continued. The series is complete, with the exception of ten days which had to be omitted. The corrective soundings at noon are continued with occasional omissions.

Series III. from April 20, 1854, to August 3, 1854. The double half-hourly readings of the pulley-gauge continue to May 5, after which date single half-hourly readings are recorded. The corrective soundings cease on May 12. Interruptions occur between May 4 and 7, on July 8, between July 15 and 18, and between July 20 and 28. On the 8th of August the brig was released from her ice-cradle and rose 2½ feet; on the 23d, the brig was in but 7 feet of water and grounded.

Series IV. from Sept. 7, 1854, to Oct. 22, 1854. The hourly observations assume again a more regular appearance; they were taken with the sounding-line and are expressed in fathoms and feet. After Oct. 21 the irregularities increase, and it was thought best to take no further notice of the remaining observations; the strength of the party no longer permitted due attention to the tidal phenomena. It was apparent that before any reduction of the tides could be made they had first to be reduced to the same zero or mean level of the sea. To effect this in a manner apparently best suiting the case and otherwise unobjectionable, two curved lines were traced on the diagrams, the upper one enveloping the highest high-water ordinate of each day, the other enveloping the lowest low-water of each day; in tracing these lines some allowance was made, when necessary, for disturbing causes, so as to obtain tolerably smooth curves, cases of abrupt changes were, of course, treated accordingly. A line equidistant from these curves was assumed as representing the mean level, and when straightened out was adopted as axis of reference. The correction to refer each observation to this plane of reference was taken from the diagram. This method of treatment excludes necessarily (in series I. II. III.) any discussion of the mean level of the sea, the oscillations of which have been found small at other places. As an illustration of this the tides at Singapore might be referred to; the Rev. W. Whewell (7th

series of researches on the tides, Phil. Trans. Roy. Society, p. I. 1837), finds for these tides, that if a line is drawn representing the mean height (midway between high and low-water each day), it is very nearly constant, though the successive low-waters often differ by six feet (on account of the diurnal inequality), the mean level only oscillates through a few inches. The use of the soundings intended to furnish corrections to the readings of the pulley-gauge is in many cases a doubtful remedy on account of the continued change in the zero of the wheel's index; in fact, to be effective, it would have required numerous soundings at other hours than noon. As it is, a combination of the corrections by enveloping curves and soundings had to be adopted. For intermediate hours the corrections as given by the curves serve as guides. The reduction to the same level affects the times generally very little.

The following note of February 3, 1854, is instructive in regard to the effect of the tides on the ice floe. "The enormous elevation of the land ice by the tides has raised a barrier of broken tables seventy-two feet wide and twenty feet high between the brig and islands. This action has caused a recession of the main floe; our vessel has changed her position twenty feet within the last two spring tides, and the hawser connected with Butler Island parted with the strain." After giving the tidal record in a form ready for use, the observations were properly tabulated for the purpose of deducing empirically their laws, and for comparison with theory. In the U. S. Coast Survey two blank forms are in use for this tabulation; they have in their essential part been adopted as suitable for the Van Rensselaer Harbor tides, and were used with permission of the Superintendent of the Survey. These forms, however, strictly apply only for such cases where the diurnal inequality is comparatively small, or is at least not approximating to the production of single day tides. In order to show, at a glance, the general character of the tides, they were replotted, when it appeared that the diurnal inequality is not of so great an amount as to render the use of the ordinary method of reduction unavailable; on the other hand, this inequality is sufficiently large to require a special discussion for time and height. The extension of the series of observations over a whole year must be considered as a fortunate circumstance, since the results thereby gain considerably in accuracy over others deduced only from a few disconnected lunations. The first form of tabulation contains, in

column 1, the date, civil reckoning; column 2 gives the apparent time of the moon's superior and inferior transit over the Van Rensselaer Harbor meridian, obtained by adding nine minutes to the time of transit at Greenwich, allowing for a difference of longitude of $4^{h.}\ 43\frac{1}{2}^{m.}$ W. Mean time was converted into apparent time by applying the equation of time. The time of the lower transit was obtained by taking the mean of the times of the preceding and following upper transit; columns 3 and 4 contain the apparent time of high and low-water taken from the record; columns 5 and 6 exhibit the luni-tidal interval between the time of high and low-water, and the time of the transit of the moon immediately preceding, though in some cases owing to the half-monthly inequality, it may be the second preceding, the establishment being about $11\frac{3}{4}$ hours. This transit of comparison has been called transit F by Sir John Lubbock. The next columns, 7 and 8, give the height of high and low-water extracted from the abstract. The remaining columns contain the moon's parallax and declination at noon. The second form of reduction is especially arranged to obtain the establishment and the half-monthly inequality in time and height; that the inequality in time and height should also be made out from the low-waters is especially important for stations where either the observations are of short extent or else where difficulties tend to render the observations less accurate.

In order to obtain well-balanced mean values in Table No. 2, Peirce's criterion was applied. Of 982 observations of the interval, only seventeenvalues were rejected.

Half-monthly inequality. — For the comparison of the observed with the theoretical values, it is customary to use the forms of the equilibrium theory or of the wave theory, certain modifications being necessary to produce an agreement between these theories and observation. According to the equilibrium theory, and allowing for the retroposition of the theoretical tide as required by the wave theory, the inequality in time can be expressed by the formula,

$$\tan 2\,(\theta^1 - \lambda^1) = -\frac{h \sin 2\,(\varphi - \alpha)}{h^1 + h \cos 2\,(\varphi - \alpha)},*$$

* Phil. Trans. Roy. Soc. 1834, Part I. On the empirical laws of the tides in the port of London, with some reflections on the theory, by the Rev. W. Whewell. See also Trans. of 1836, Part I., on the Liverpool tides, by the same author.

where h and h^1 are the elevations of the aqueous spheroid due to the sun and moon respectively, φ the angular distance of the moon from the sun, θ^1 the angular distance of the pole of the spheroid (or of high-water), from the moon's place. The pole of this spheroid follows the moon at a certain distance, the mean value λ^1 of which is known as the "mean establishment," and which corresponds to a distance of the sun and moon of $\varphi - \alpha$ instead of φ. This inequality goes through its period twice in each month. The observations of 480 high-waters and 485 low-waters furnished the following tables of the luni-tidal intervals; they are given separate for upper and lower transit of the moon, but in this abstract I present only the mean values:—

From observed high-waters.			From observed low-waters.		
Apparent Solar time of Moon's transit.	Luni-tidal Interval.	No. of Observations.	Apparent Solar time of Moon's transit.	Luni-tidal Interval.	No. of Observations.
h. m.	h. m.		h. m.	h. m.	
0 29	11 37	43	0 30	17 50	45
1 30	11 12	43	1 31	17 48	42
2 30	11 01	40	2 30	17 29	41
3 30	10 47	45	3 30	16 59	46
4 30	10 56	41	4 30	16 49	39
5 29	10 58	36	5 29	17 00	33
6 32	11 59	36	6 30	17 40	37
7 30	12 27	38	7 29	18 17	40
8 30	12 43	45	8 30	18 31	48
9 31	12 22	36	9 31	18 43	35
10 30	12 21	39	10 30	18 19	42
11 30	12 16	38	11 30	18 11	37
Mean and sum,	11 43.3	480	Mean and sum,	17 48.0	485

The mean establishment resulting from the observed times of 480 high-waters is therefore $11^{h.}\ 43.3^{m.}$ referred to the moon's transit immediately preceding and corresponding to a mean horizontal parallax of the moon and sun, and to the moon's (and sun's) declination of 15°. This mean interval from the high-waters corresponds to the moon's transit of $0^{h.}\ 21^{m.}$ nearly, indicating that the epoch would have come out $0^{h.}\ 0^{m.}$ if transit E (of Lubbock's) had been used. The difference between the establishment of the high low-water is $6^{h.}\ 04.7^{m.}$. We find from the observed high-waters $\alpha = 0^{h.}\ 21^{m.}$ and from the low-waters $0^{h.}\ 50^{m.}$. Range of the inequality from the high-waters $1^{h.}\ 51^{m.}$, and from the low-waters $1^{h.}\ 54^{m.}$, hence the expression for the half-monthly inequality becomes:

From high-waters:

$$\tan 2\,(\theta^1 - 175^\circ\ 49'.5) = -\frac{0.4649 \sin 2\,(\varphi - 5^\circ\ 15')}{1 + 0.4649 \cos 2\,(\varphi - 5^\circ\ 15')}.$$

From low-waters:

$$\tan 2\,(\theta^1 - 267^\circ\ 00') = -\frac{0.4771 \sin 2\,(\varphi - 12^\circ\ 30')}{1 + 0.4771 \cos 2\,(\varphi - 12^\circ\ 30')}.$$

The observed and computed values agree very closely, as may be seen from the diagram. From the times we have the mean value

$$\frac{h}{h^1} \text{ or } \frac{S''}{M''}$$

of the wave theory, and

$$(A) \text{ of Lubbock's} = 0.471 \text{ and } \alpha = 0^{h.}\ 36^{m.}$$

Hence the age of the tide or the time requisite for the moon to increase its right ascension by that amount becomes eighteen hours.

Half monthly inequality in height. The theoretical expression is,

$$y = \sqrt{[h^{1\,2} + h^2 + 2\,h^1\,h \cos 2\,(\varphi - \alpha)]}.$$

The following table contains the results of the observations from the high and low-waters (the mean from the moon's superior and inferior transit):

Moon's transit.	Height of high-water.	Number.	Height of low-water.	Number.
h. m.	Feet.		Feet.	
0 30	12.1	45	1.4	45
1 30	11.9	43	1.2	43
2 30	11.8	39	1.7	40
3 30	11.0	47	2.0	46
4 30	10.5	42	3.2	41
5 30	9.5	38	4.1	35
6 30	9.1	39	4.4	40
7 30	9.3	39	4.7	42
8 30	9.8	47	3.8	49
9 30	10.4	38	2.8	36
10 30	11.0	40	1.9	42
11 30	11.6	38	1.4	38
Mean and sum,	10.67	495	27.2	497

The inequality is closely represented by the expression,

$$\text{For high-water, } y = 10.6 + 1.5 \cos 2 (\varphi - 15^\circ),$$
$$\text{" low " } y^1 = 2.7 - 1.7 \cos 2 (\varphi - 15^\circ),$$

the maximum difference between observed and computed values is 0.4 feet; the small differences are shown by the diagram. From the inequalities in height

$$\frac{h}{h} \text{ or } \frac{S'''}{M'''} = 0.367.$$

The ratio of the solar to the lunar tide is deduced with more exactness from the inequality in times, and the above value is certainly greater than the average value deduced at more southern stations. One of the reasons why this ratio is not constant, and which probably applies here, may be stated as depending on the approach of the tides by opposite channels; in which case this ratio depends on the length of the channels; this view would require a polar tide to enter through Kennedy Channel to combine with the principal tide which passes up Baffin's Bay and enters by Smith's Straits. According to the equilibrium theory, there should be no tide at the pole, and but a small tide in lat. 78½, but it is the tide wave propagated from the Atlantic which may be felt in this part of the polar regions. With regard to α, its value as found by the heights is more accurate than that found by the times, the latter gave $\alpha = 9^\circ$, the former 15°; adopting 15°, the retard or age of the tide becomes 1¼ day, by which interval the spring and neap-tides follow syzygies and quadratures respectively. The time value of α is here smaller than the height value, which is more in accordance with theory than the opposite. We have also, mean rise and fall of tides at Van Rensselaer Harbor 7.9 feet, range of spring tides 11.1 feet, and range of neap-tides 4.7 feet. These numbers are averages from the discussion of 9½ lunations, and obtain without regard to the diurnal inequality.

Effect of the changes in the moon's declination and parallax on the half-monthly inequality in time. To ascertain the effect due to the changes in the moon's declination and parallax, an anterior value, corresponding to a certain age of the tide is to be used in the comparison; the preceding investigation gave for the retard 1¼ day; each luni-tidal interval minus its corresponding mean value for the respective hour of

transit, was tabulated, together with the moon's declination and parallax (each separately) and corresponding to one day anterior to the time of high or low-water. The observations, while they give the half-monthly inequality with considerable accuracy, cannot be expected to give its variations with the same reliance, and it was found necessary to form groups for different values of the declination and parallax. The following table of corrections was obtained: —

CORRECTION TO INTERVAL FOR MOON'S DECLINATION.

Moon's transit.	0° to 13°	13° to 21°	21° to 27°.5
h. m.	m.	m.	m.
0 30	— 2	— 7	+ 5
1 30	+15	— 5	—18
2 30	+21	— 7	—12
3 30	+23	—11	— 5
4 30	+ 9	0	—13
5 30	+14	+ 4	—15
6 30	— 9	+18	— 3
7 30	+ 1	—13	+ 9
8 30	— 2	+ 9	+15
9 30	— 6	+ 1	+22
10 30	+ 1	— 6	+10
11 30	— 3	— 2	+ 3
Mean,	+ 5	— 1	— 1
Number of Observations,	373	262	348

CORRECTION TO INTERVAL FOR MOON'S PARALLAX.

	54′ to 56′	56′ to 58′	58′ to 61′.4
	m.	m.	m.
	—12	+14	— 7
	—17	+17	+ 5
	—11	+12	— 2
	— 1	+20	—10
	— 1	+ 1	— 3
	+16	+19	—36
	+ 7	— 3	— 2
	+ 5	— 3	— 6
	+26	—10	+ 8
	+ 9	—11	— 5
	+10	— 9	— 7
	— 4	+ 4	+ 4
Mean,	+ 2	+ 4	— 5
Number of Observ'ns,	387	244	333

The table exhibits systematical values for the periodical part of the lunar effect or for the term $D \sin 2 (\varphi - \gamma)$ referring to the declination changes. D is about $14^{m.}$, and γ is about 15°, 60°, and 105° respectively. The variation in the inequality due to the moon's declination appears large when compared with its value at other places, but it is in conformity with the large value of half-monthly inequality itself.

Diurnal inequality. We now proceed to the examination of a prominent feature of the Van Rensselaer tides, namely, the diurnal inequality. The laws of this inequality were first understood and reduced to computation by the Rev. W. Whewell, and the subject has since been taken up by Prof. A. D. Bache, superintendent U. S. Coast Survey. His researches commenced about nine years ago, and resulted in a further extension of the method of discussion as well as in the recognition of the geographical extent of the phenomenon on our own coast, single day tides. The effect of this inequality in extreme cases was now successfully discussed and reduced to rule; for these labors the reader may be referred to the Phil. Trans. 1836 and 1837, and to Coast Survey reports of 1851, 1852, 1853, 1854, and 1856. According to the equilibrium theory, the diurnal tide ought to be very small in latitude 79°; but viewing the Van Rensselaer harbor tide as a consequence of the propagated Atlantic tide, the existence of a diurnal inequality in so high a latitude need not surprise us. The following notes were extracted from Capt. (now Sir) F. L. McClintock's narrative of the voyage of the Fox, 1857, 1858, 1859. Referring to Bellot Straits: "As in Greenland, the night tides are much higher than the day tides." Speaking of the ice motion, he says: "Now we know that the night tides in Greenland greatly exceed the day tides." When near Buchan Island the vessel grounded during the day tide, and was floated off by the night tide. By the labors of Dr. Kane we now know that the diurnal inequality is found as high up as latitude 79°. At Northumberland Island the difference in the day and night tide was found to be three feet. A cursory examination of the plates showed that this inequality is well marked in the high-waters, but less so in the low-waters; that sometimes the day tide, at other times the night tide, is the higher of the two occurring in a lunar day; further, that it vanishes a day or two after the moon's crossing the equator, and that it amounts in maximo to about three feet some time after the moon attains her greatest

declination. The diurnal inequality is produced by the interference of two independent waves, the diurnal and the semi-diurnal, — the former depending for its size chiefly for the moon's declination. The less rigorous method of discussion as used by Mr. Whewell was applied to the greater number of the observations, and the application of the more refined process by graphical separation as practised on the coast survey was reserved for the last series. Following the first method the observed heights of high and low-water were laid down graphically, and a line was drawn by the eye cutting off the zigzags of the successive high-waters, leaving equal portions above and below the intermediate curve. These differences from the mean height were then set off from another axis, and those belonging to the high-water next following the moon's superior transit were marked by a curve of dashes, those following the moon's inferior transit were marked by a curve of dots. These curves without exception were found to have alternately, as the moon has a north or south declination, positive and negative ordinates in perfect accordance with the equilibrium theory, according to which the tide (high-water) which belongs to a south transit of the moon should be the greater of the two of the same day, the moon's declination being north; or should be the smaller of the two, the moon having south declination; when the moon crosses the equator (or according to experience, some time after it), the inequality vanishes, the number of days by which the cause precedes the effect was found on the average 1d. 15h. The magnitude of the diurnal inequality and its variation depending on twice the moon's declination was made out by dividing the inequality curves into six parts between the times of disappearance; the following was the result: —

DIURNAL INEQUALITY IN HEIGHT.

Moon's declination.	Observed depth; feet.	Computed depth; feet.	
0°	0.0	0.0	Epoch 1.6 days
12	1.4	1.4	$dh = C \sin 2\delta^1$
21	2.3	2.2	$C = 3.3.$
25	2.5	2.5	
22	2.3	2.3	
13	1.7	1.5	
0	0.0	0.0	

The diurnal inequality in time I have tried to discuss by calculation and a graphical process, but owing to the fact that the observations were only made half hourly and at other times hourly, the irregularity searched for could not be traced satisfactorily. It is probably not exceeding two hours, and seems to be less in amount for the times of high-water than for the times of low-water, a result the reverse of that belonging to the irregularity in height. A similar conclusion was arrived at in the discussion of the tides at San Francisco, Cal., by Prof. A. D. Bache. The actual separation of the semi-diurnal and the diurnal wave has been effected graphically for the period from Oct. 30 to Nov. 22, 1853. The process may be briefly explained as follows. After the observations are plotted and a tracing is taken, the traced curves are shifted in epoch twelve (lunar) hours forward when a mean curve is pricked off between the observed and traced curves; this process is repeated after the tracing paper has been shifted twelve hours backward; the average or mean pricked curved thus obtained represents the semi-diurnal wave. On an axis parallel to that on which the time is counted, the differences between the originally observed and the constructed semidiurnal curve were laid off; this constitutes the diurnal curve. The epoch of high water of the two curves was found nearly to be coincident.

Form of the tide wave. The shape of the tide wave has been ascertained in the manner described in art. 479, Tides and Waves, Encyclopædia Metropolitana, and the result depends on the hourly observations of sixty tides, thirty during spring tides and an equal number during neap-tides. The observed heights on the day of the syzygies and quadratures, and on the first and second day after, were tabulated, forming ten groups of three columns each. The time elapsed from low-water to the neat low-water was taken to correspond to 360° of phase. It was found that the spring tide wave is slightly steeper between low and high-water than between high and low-water, and that the neap-tide wave is very nearly symmetrical in respect to rise and fall.

The duration of rise is $6^{h.}\ 04.7^{m.}$, hence the duration of fall, $6^{h.}\ 19.7^{m.}$ The form of the wave has been represented by

(Spring tide) $5.83 + 5.58 \sin(\theta + 278°) + 0.20 \sin(2\,\theta + 281°)$

(Neap-tide) $2.42 + 2.25 \sin(\theta + 269°) + 0.09 \sin(2\,\theta + 290°)$.

The angle θ counts from low-water, and the height of the wave is ex-

pressed in feet. The agreement between the computed and observed shape is very close. Respecting the effect of the wind and ice on the tides it may be remarked that the former can only be slight, since the sea is protected from direct action by its icy cover during the greater part of the year; the ice crust cannot possibly affect (by friction on its lower surface), the progress of the tide wave, and will certainly not sensibly interfere (by friction on the ice-foot and breakage of the ice-fields) with the rise and fall of the tide.

Progress of the tide wave.—The tide at Van Rensselaer Harbor may be taken as a derived tide and transmitted to it from the Atlantic Ocean, and in part modified by the small tide originating in the waters of Baffin's Bay, which latter tide however must necessarily be small, particularly on account of the general direction of the Bay, which is very unfavorable for the production of a tide wave. That the tide wave runs up the western coast of Greenland, or in other words reaches Van Rensselaer Harbor from the southward, may be seen from the following observed establishments:—

Holsteinborg, lat. 66° 56′, long. 53° 42′. High-water F. & C. VI^h. XXX^m. Spring tides rise 10 feet.

Whalefish Islands, lat. 68° 59′, long. 53° 13′. Time of high-water F. & C. VIII^h. XV^m. Highest tide 7½ feet.

Godhavn, lat. 69° 12′, long. 53° 28′. Tidal hour IX^h. Rise and fall 7½ feet.

Upernavik, lat. 72° 47′, long. 56° 03′. High-water at F. & C. XI^h. Rise 8 feet.

Wolstenholm Sound, lat. 76° 33′, long. 68° 56′. High-water at F. & C. XI^h. VIII^m. Rise 7 to 7½ feet.

Van Rensselaer harbor, lat. 78° 37′, long. 70° 53′. High-water at F. & C. XI^h. L^m. Average range 7.9 feet.

The tidal observations at Wolstenholm Sound, taken by Capt. Saunders, of H. M. ship North Star, 1849 and 1850, were kindly furnished to Prof. Bache, by the hydrographer to the admiralty.

By means of the differences in the establishments of these several places, corrected for local time and moon's motion, and Airy's table, 174, Tides and Waves, we can obtain an approximation to the depth of Baffin's Bay and Smith's Straits. Between Holsteinborg and Van Rensselaer I find average depth of 220 fathoms, an inferior limit; between Upernavik and Van Rensselaer the average depth is near 800 fathoms, and a similar result from a combination with Wolstenholm; this latter value is perhaps a superior limit. In lat. 51° 12′, long. 52° 8′ a sounding was taken and bottom obtained with 178 fathoms. In

lat. 59° 48′, long. 50° 03′, bottom was found with 1,817 fathoms; in Melville Bay, lat. 75° 40′, long. 62° 12′, bottom was found with 429 fathoms, dark green sand.

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5. General Account of the Results of Part II. of the Discussion of the Declinometer Observations, made at the Girard College, Philadelphia, between 1840 and 1845, with Special Reference to the Solar-diurnal Variation and its Annual Inequality. By A. D. Bache, Superintendent U. S. Coast Survey.

The discussion presented last year to the Association embraced the amplitude of the solar-diurnal magnetic variation, as well as that of the number and magnitude of the disturbances of declination, in reference to the eleven years' period heretofore pointed out by General Sabine and others. The discussion now presented, is of the regular solar-diurnal variation and its annual inequality. The complete results of the computations, as in the other case, will be given in the Smithsonian Contributions to Knowledge, and the present paper will contain merely a general account of the results.

The normals, or means freed from the disturbances are used in the discussion, avoiding thus the necessity for rejecting the observations of months in which disturbances are frequent. The same course is followed by the Rev. Prof. H. Lloyd in his discussion of the Dublin observations, and also by Gen. Sabine in the third volume of the discussion of the Toronto observations. I therefore return to the hourly normals given in Part I. of the discussion, and arrange them according to months of the year, correcting those of 1845 for index error as already explained. The tables given in the complete memoir show at one view the mean hourly readings for each month unaffected by the large disturbances, and the mean hourly position of the magnet in reference to its general mean position. The solar diurnal variation for each month is readily traced in the table, but the annual inequality in the diurnal variation, being affected by the secular changes, is less distinctly traceable. To give this result distinctness, each hourly normal is compared with the corresponding monthly mean value, as set down

Mean Solar d

the last vertical column of the table, and the results are recorded in ew table. The sign + in this latter table indicates a westerly, and sign — an easterly deflection of the north end of the magnet from mean position, the scale divisions having been converted into min- s of arc.

The distinctive features of this second table are brought out both lytically and graphically. The inequality in the diurnal variation most readily seen by comparing the horizontal lines in the table n August and February, and the annual variation appears most nly by carrying the eye down the vertical columns for the hours of nd 7 A. M.

The annual variation depends upon the earth's position in its orbit, diurnal variation being subject to an inequality depending upon the 's declination. The range is greater during the summer, when the lination is north, and less during the winter when the declination is th, and passes from one to the other about the time of the equinoxes. e summer and winter means are therefore tabulated for comparison th the general mean. Diagram A shows the type curves for the mmer and winter periods and the general mean of the year. The mmer months, April to September inclusive, give a diurnal range of rly $10\frac{1}{2}$ minutes, and the winter months one of $5\frac{1}{2}$ minutes.

Diagram B. shows the same result in a different form, the mean arly curve of the first diagram (A.) being straightened out to form e axis of the second (B.). The curves represent the winter and summer variations, the ordinates being the difference between those of the yearly curve, and of the winter and summer curves respectively. This diagram shows very perspicuously the progress of the annual variation at different hours of the day. It shows that at 6 or 7 A. M. the annual variation is a maximum disappearing at a quarter before 10 A. M. That it reaches a second (secondary) maximum at 1 P. M., nearly disappearing shortly after 5 P. M. A still smaller maximum is reached after 9 P. M., and half an hour before midnight the annual variation again disappears. At, and before and after the principal maximum between 6 and 7 A. M. the annual variation causes the north end of the magnet to be deflected to the eastward in the summer and to the westward in the winter. At 1 P. M. the deflections are to the west in summer and to the east in winter, the range of diurnal motion being thus increased in the former season and diminished in the latter. The

needle is thus deflected in the summer more to the east in the morning hours and more to the west in the afternoon hours, or has greater elongation than it would have if the sun moved in the equator. In winter the reverse is the case. The range of annual variation from summer to winter at Philadelphia is about 3′, and its daily range about 2′.6.

In diagram C. similar curves are given for Philadelphia, St. Helena, Toronto, and Hobarton, of the winter and summer progress of the annual variation. The comparison indicates that Toronto and Hobarton are not normal types of the half-yearly deflections, and the near coincidence of the forms at Philadelphia and St. Helena seems to show that the type for different places is one and the same in general character affected by incidental irregularities.

In reference to the annual variation, Gen. Sabine, in the rectifications and additions to the last volume of Humboldt's Cosmos, expresses himself as follows: —

"Thus in each hemisphere the semi-annual deflections concur with those of the mean annual variation for half the year, and consequently augment them and oppose and diminish them in the other half. At the magnetic equator there is no mean diurnal variation, but in each half year the alternate phases of the sun's annual inequality constitute a diurnal variation of which the range in each day is about 3′ or 4′, taking place every day in the year, except about the equinoxes, the march of the diurnal variation being from east in the forenoon to west in the afternoon, when the sun has north declination, and the reverse when south declination."

According to the same authority, the *annual* variation is the same in both hemispheres, the north end of the magnet being deflected to the east in the forenoon, the sun having north declination, whereas in the *diurnal* variation the north end of the magnet at that time of the day is deflected to the east in the northern hemisphere, and to the west in the southern. In other words in regard to direction, the law of the annual variation is the same as that of the diurnal variation, but opposite, in passing from the northern to the southern magnetic hemisphere.

Some interesting conclusions as to the law of change of the annual variation will flow from a closer discussion of the observations at the two hours of maximum, namely, 6 to 7 A. M., and 1 to 2 P. M. The general table, by subtracting the annual mean from each monthly value,

at the respective hours, gives the following values for the annual variation at or near the hours of principal and secondary maxima of range, the signs + and — respectively, indicating as usual westerly or easterly deflection from the annual mean position:—

	6 to 7 A. M.	1 to 2 P. M.		6 to 7 A. M.	1 to 2 P. M.
	′	′		′	′
January	+2.01	—0.98	July	—1.94	+1.25
February	+1.31	—1.12	August	—2.66	+1.31
March	+0.47	—0.47	September	—1.24	+0.82
April	+0.15	+1.06	October	+1.41	—1.14
May	—1.38	+0.93	November	+1.47	—1.35
June	—1.90	+0.89	December	+2.30	—1.20

The greatest range at 6 to 7 A. M. is 5′.0, the easterly deflection being greater than the westerly by 0′.4. That from 1 to 2 P. M. is 2′.7, the eastern and western deflections being equal. A general inspection of the columns shows that the solstices are approximately the turning epochs of this annual variation, and that the signs change at the time of the equinoxes. To determine these points with more precision the numbers of the table were expressed by an analytical formula. According to this, January 1 and July 1 (ten days after the solstices), are the dates of the greatest values, and the transitions from positive to negative values, and the reverse will occur on the 1st of April and the 1st of October (ten days after the equinoxes). A table is given in the memoir showing the satisfactory coincidence of the observed and computed values. This result agrees with that deduced by a different method by Gen. Sabine.

To give a definite determination of the law of the phenomenon, so as to embrace the whole progress shown by the series, the regular solar diurnal variation has been expressed as a function of the time, by four terms of Bessel's formulæ, the equation being found for each month and also for each half year from April to September, and from October to March, and also for the whole year. Allowance was made in determining the coefficients for the different weight of the readings at the even and at the odd hours.

The equation for the whole year is compared with that given by Prof. Lloyd for Dublin. Reference is made to the monthly equations

deduced by Mr. Karl Kreil from a consecutive series of observations made at Prague from 1840 to 1849, and selected from a longer series of thirteen years.

The following table exhibits the close correspondence of the computed and observed mean annual value of the regular solar diurnal variation: —

Phila. Mean Time.	Diurnal Var'n.		Diff.	Phila. Mean Time.	Diurnal Var'n.		Diff.	Phila. Mean Time.	Diurnal Var'n.		Diff.
	Com'd.	Obse'd.			Com'd.	Obse'd.			Com'd.	Obse'd.	
h. m.	′	′	′	h. m.	′	′	′	h. m.	′	′	′
0 19½	—0.49	—0.47	—0.02	8 19½	—3.44	—3.50	+0.06	16 19½	+1.71	+1.80	+0.09
1 "	—0.48	—0.52	+0.03	9 "	—2.29	—2.43	+0.14	17 "	+0.88	+0.78	—0.10
2 "	—0.51	—0.44	—0.07	10 "	—0.24	—0.19	—0.05	18 "	+0.33	+0.25	—0.08
3 "	—0.67	—0.71	+0.04	11 "	+2.03	+2.17	+0.14	19 "	—0.07	—0.07	—0.00
4 "	—1.09	—1.19	+0.10	12 "	+3.69	+3.65	—0.04	20 "	—0.38	—0.33	—0.05
5 "	—1.82	—1.64	—0.18	13 "	+4.28	+4.32	+0.04	21 "	—0.57	—0.60	+0.03
6 "	—2.77	—2.72	—0.05	14 "	+3.81	+3.77	—0.04	22 "	—0.62	—0.64	+0.02
7 "	—3.49	—3.47	—0.02	15 "	+2.77	+2.76	—0.01	23 "	—0.57	—0.71	+0.14

The greatest difference at any one hour is less than 11″, and the probable error of any single computed value is ± 0′.19. Diagrams D. and E. give the resulting curves for the computed hourly values of the diurnal variation for each month of the year. Diagram D. contains the curves for the six months of the summer half year, and E. for the six months of the winter half year. Positive ordinates correspond as before to a westerly motion of the north end of the needle, and negative ordinates to an easterly motion. Diagram F. contains the type curves for the summer, winter, and the whole year.

From the computed values as given in the table, assisted by the diagrams, the general features of the diurnal variation and of its annual inequality are readily deduced.

The general character of the diurnal motion is nearly the same for the summer half year, for the winter half, and therefore for the whole year. The greatest eastern deflection is, at a mean, reached at a quarter before eight, A. M., being a quarter of an hour earlier in the summer, and half an hour later in the winter. Near this hour the declination is a minimum. The greatest western deflection is reached, at a mean, at a quarter after one o'clock, P. M., a few minutes earlier in both the summer and winter. At this hour the declination is a maximum. The

diurnal curve presents but a single wave, slightly interrupted by a deviation occurring during the hours near midnight or from 10 P. M. to 1 A. M., when the magnet has a direct or westerly motion. Shortly after 1 A. M., the north end of the magnet moves easterly, completing the cycle, and arriving at its eastern elongation shortly before 8 A. M. This nocturnal deviation is well marked in winter, vanishes in summer, and is but slightly perceptible in the annual curve.

According to the investigations of Gen. Sabine, it is probable, that if the effect of disturbances was completely obliterated in the results, this small oscillation would disappear. In summer, when it is not noticed, the needle remains nearly stationary from 8 P. M. to 3 A. M. The type curves for the whole year show a similar result. The diurnal curves for the months when the sun's declination is north and those when it is south, resemble each other closely, as is shown by diagrams D. and E.

For greater precision in regard to the epoch and amount of the diurnal variation we must recur to the analytical expressions representing the numbers of the table.

The following table contains the results for each month, and for the summer and winter seasons, and the whole year, also the critical interval between the two adjacent hours of the mean positions:—

Month.	Eastern Elong'n. A. M.	Western Elong'n. P. M.	Critical Interval from Min. to Max.	Epoch of mean declination.		Critical Interval.
				A. M.	P. M.	
	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.
January	8 58	1 27	4 29	10 52	7 08	8 16
February	8 34	1 32	4 58	10 52	7 26	8 34
March	8 07	1 34	5 27	10 46	7 32	8 46
April	8 12	1 27	5 15	10 34	7 40	8 56
May	7 29	1 21	5 52	10 19	6 57	8 38
June	7 33	1 20	5 47	10 25	8 26	10 01
July	7 36	1 28	5 52	10 30	9 32	11 02
August	7 18	1 05	5 47	10 10	8 40	10 30
September	7 30	0 45	5 15	9 58	6 45	8 47
October	8 00	1 17	5 17	10 30	5 23	6 53
November	7 54	1 08	5 14	10 16	6 08	7 52
December	8 54	1 40	4 46	10 50	6 17	7 27
Summer	7 33	1 08	5 35	10 17	7 43	9 26
Winter	8 24	1 25	5 01	10 40	6 49	8 09
Year	7 48	1 16	5 28	10 26	7 08	8 42

The formulæ also give for the time of the secondary minimum of eastern declination in winter 9h. 42m. P. M., and for its amount — 0′.97.

For the time of the secondary maximum of western declination in winter 1h. 15m. A. M., and for its amount — 0′.26. Differences 3h. 33m. and 0′.71.

For the secondary minimum of eastern deflection for the year, 10h. 11m. P. M., and its amount — 0′.62.

For the secondary maximum of western deflection for the year, 1h. 13m. and its amount — 0′.47. Differences 3h. 02m. and 0′.15.

The effect of the season on the critical hours is well marked in the foregoing table, the eastern elongation occurring earliest between the summer solstice and the autumnal equinox, and latest about the winter solstice. The western elongation occurs earliest about the autumnal equinox, and latest about the winter solstice, and the same holds good for the morning epoch of the mean declination. The afternoon epoch, however, occurs earliest shortly after the autumnal equinox, and latest shortly after the summer solstice. The critical hours which are most constant during the year are those of the western elongation and of the morning mean declination. The greatest difference between any month and the mean of all the months, is 31 minutes in the former, and 28 in the latter.

To exhibit the features of the diurnal variation and its annual inequality, a graphical representation is given in diagram C. The magnetic surface is formed by contour lines 0′.5 apart. The curves in dots (. . . .) are lines of mean position. Those in dashes (— — — —) are eastern deflections from the normal positions, and the full lines are western deflections. This diagram and the table from which it is deduced are immediately applicable to the practical problem of furnishing the correction to be applied to a single observation made at any hour of the day and month to reduce it to its mean value. It also renders unnecessary the development of the annual variability of the coefficients in the analytical expression. The diagram distinctly exhibits the diurnal minima and maxima, the former represented by a valley, the latter by a ridge on the magnetic surface.

Next, the magnitude of the diurnal range is studied. The following table contains the amount of the deflection at the eastern and western elongation, and the diurnal amplitude of the declination for each month of the year derived from the equation :

	Deflection at East Elong.	Deflection at West Elong.	Diurnal Range.		Deflection at East Elong.	Deflection at West Elong.	Diurnal Range.
	′	′	′		′	′	′
January	—2.46	+3.52	5.98	July	—5.58	+5.46	11.04
February	—2.64	+3.11	5.75	August	—5.79	+6.36	12.15
March	—3.73	+4.03	7.76	Sept.	—4.71	+5.60	10.31
April	—4.02	+5.28	9.30	October	—2.18	+3.23	5.41
May	—4.89	+5.16	10.05	November	—1.92	+2.85	4.77
June	—5.26	+5.06	10.32	December	—1.65	+3.14	4.79

The diurnal range for the summer months is 10′.45; for winter months, 5′.56; and for the whole year, 7′.89: all corresponding to an epoch about a year and a half removed from the epoch of a minimum of the solar period.

The numbers denoting the diurnal range exhibit three remarkable features, namely, the maximum value in August, the sudden falling off in September and October (diagram H.), and the minimum value in November and December. In other respects the progression is regular. The curve is single crested, which holds in the eastern as well as in the western deflections when viewed separately. This is of special importance, as it is probable that the interference of these separate curves at other stations chiefly determines the double crested character of the curves of diurnal range. The curves for Milan, Munich, Göttingen, Brussels, Greenwich, Dublin, etc., for instance, exhibit two maxima, one after the vernal equinox, and a second, generally the smaller, about the summer solstice. The system to which Philadelphia belongs is exemplified by the annual progress of the diurnal range at Prague and certain Russian stations, as at Nestchinsk, but more closely by Toronto, as seen in diagram H. Neither station appears to have a tendency to a secondary maximum about the month of April, so that the maximum about a month and a half after the summer solstice is a well-marked feature of the North American stations.

In connection with the preceding discussion the inequality in the magnetic declination from year to year next claims attention. The subject presents greater inherent difficulties than the diurnal inequality, on account of the difficulty of keeping the instruments in precisely the same condition of adjustment throughout the year. In the first part of the discussion I had occasion to refer to this circumstance while investi-

gating the annual effect of the secular change, and it was then shown that the Philadelphia observations share in this respect a similar difficulty with other stations, in consequence of which the results must be received with caution.*

In the mean monthly values of the declinometer readings and in their differences when compared month for month, are combined the joint effect of the secular change and of the annual inequality. To eliminate the effect of the secular change, conditional equations were formed which give a value, deduced by the application of the method of least squares of 1.227 divisions, as the monthly effect of the secular change.† The effect of the secular change was deduced from the mean monthly values, and these values themselves being subjected to certain corrections which the numbers show to be necessary, the following result for the annual inequality was obtained:

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
Mean declinometer readings,	563.8	563.1	563.5	562.6	561.1	562.2	562.1	561.2	558.4	558.1	554.6	554.4
Correc'n for secular change,	−4.4	−3.6	−2.8	−2.0	−1.2	−0.4	+0.4	+1.2	+2.0	+2.8	+3.6	+4.4
Annual variation in arc,	′ +0.5	+0.4	−0.1	−0.1	+0.2	−0.6	−1.0	−0.9	0.0	−0.2	+0.9	′ +0.7

This result has been adopted as representing the annual variation. From June to October the north end of the magnet is accordingly to

* Dr. Lloyd's instructive note on this subject in his discussion of the Dublin observations, is as follows:—"The determination of the annual variation is much more difficult than that of the diurnal, both on account of the much smaller frequency of the period and the difficulty of preserving the instrument in the same unchanged condition during the much longer time, or of determining and allowing for its changes when they do occur; accordingly, although the annual period may be traced in the observations of Gilpin and is decidedly displayed in those of Bowditch, it has evaded the researches of recent observers. There is but a faint indication of its existence in the Göttingen observations, which were made at the hours of 8 A. M. and 1 P. M., and Prof. Gauss and Dr. Goldschmidt find, in the analysis of these observations, no important fluctuations dependent on season. A similar negative result is deduced by Dr. Lamont from the Munich observations, which were made twelve times a day."

† This value, 6′.7 of annual change, though not preferable to the value (4′.5) deduced by a different method in Part I. of this discussion, must necessarily be employed in the present investigation. The most reliable value 5′.0 was deduced from independent observations, as already remarked.

the eastward of the mean annual position (after the elimination of the secular change), and in the remaining months of the year it is to the westward of this position. From the vernal equinox until after the summer solstice, the motion is to the eastward or retrograde in regard to the advance of the secular change (to the westward). This is in conformity with the law given by Dr. Lloyd in the Dublin discussions, the motion of the magnet there being to the westward at that period of the year, or the reverse of the Philadelphia deflection. The secular change is also reversed; the west declination diminishing at Dublin between 1840 and 1843. The results for annual inequality from seven years of observation at Toronto are also brought out and compared with the Philadelphia results. The secular change at Toronto was 2′.0, whilst at Philadelphia (1843), it was 4′.4. In regard to the amount of the inequality, the two stations agree remarkably well, the range remaining slightly below 2′ of arc. It is supposed that this range at the same station is increasing or diminishing as the secular change increases or diminishes.

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6. Abstract of a Discussion of the Influence of the Moon on the Declination of the Magnetic Needle from the Observations made at the Girard College, Philadelphia, between the Years 1840 and 1845. By A. D. Bache, Superintendent of the U. S. Coast Survey.

The existence of a sensible lunar effect on the magnetic declination has already been established by the labors of Brown, Kreil, Sabine, and others. It is nevertheless important to add the weight of new numerical results to those already obtained.

In the discussion of the Philadelphia observations of magnetic declination already presented to the Association, I have shown how the influence of magnetic disturbances, of the eleven year period of the solar diurnal variation and its annual inequality of the secular change, and of the annual variation, may be severally eliminated, leaving residuals from which the lunar influence is to be studied.

Each observation was marked with its corresponding lunar hour and the hourly normals used for comparison. This method of treatment of

the subject is that followed by General Sabine in his discussion of the results of the British observations. The details of the method will be better understood by an example.

The time of the moon's passage over the meridian of Philadelphia (upper transit) was obtained from the American Almanac, the small correction for the difference of longitude being neglected. The observation nearest to the local mean solar time of the moon's transit was marked with a zero signifying the $0^{h.}$ of lunar time. The time of the inferior transit was next obtained, and the observation nearest to it in time was marked $12^{h.}$. The greatest difference in interval between the moon's transit and the time of observation could in no instance exceed half an hour. In the bihourly series, the observation nearest the moon's transit, or to either hour angle, one hour before or one hour after the transit, was marked. The mean of a number of differences for the same hours will thus give a result corresponding nearly enough with the hour. The number of observations intermediate between those marked $0^{h.}$ and $12^{h.}$ were marked with the corresponding hour angle by interpolation, care being taken to note the nearest full hour against each observation in the bi-hourly series. The hourly series begins with October, 1843. In the case of thirteen observations within twelve lunar hours, the one nearest midway between the two consecutive lunar hours was omitted.

The month of March 1842 is selected as an example of working the bi-hourly series, and the tables are given in the complete paper which it is expected will be published in the Smithsonian Contributions to Knowledge.

One of the first questions to determine is, how many of these residuals must be used, to give a definite result? and another one is, whether numbers deduced from different parts of the series would give harmonious results? To test both of these, the observations were formed into three groups, one containing 4,900 in nineteen months of 1840 and 1841; another, 6,715 results in twenty-one months of 1842 and 1843; and a third, 10,029 results in eighteen months of 1844 and 1845; in all, 21,644 results.

The tables for the several months and for each year, showing the residuals for each lunar hour, are given in the complete memoir in detail, as also the results of the discussion of groups I. and II. and of III., in which all the observations are united. Special investigation

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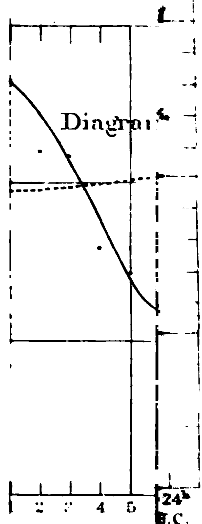

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showed that the weights were nearly proportioned to the number of observations, a result which indicates that no undue constant errors influence the result.

The results of the discussions of these three groups were also expressed by Bessel's formulæ and treated by the method of least squares. Two terms of the formulæ suffice to represent the observations, and there is no constant term indicating that the moon has no specific constant action of deflection on the needle. The coefficient of the first term is small, the character of the curve depending chiefly upon the second term which produces a double crested curve, showing two eastern and two western deflections in one lunar day. The difference in the amount of the eastern and of the western ranges depends upon the coefficient of the first term. The progression of the hourly values is systematic, and the agreement between the computed and observed values is satisfactory. On the Diagram B, the observed values are indicated by dots, and the smooth curve results from the equation.

The difference between the curves of deflection for the eastern and western hour angles shown in the curve, enables us to determine the diurnal lunar tide, and is drawn on the plate and represented by the first term of the formulæ. The interference of the two curves gives the observed form.

The curves all agree in their distinctive character, and show two east and two west deflections in a lunar day, the maxima W and E occurring about the upper and lower culminations, and the minima at the intermediate six hours. The total range hardly reaches 0′.5. These results agree generally with those obtained for Toronto and Prague. From eight thousand to ten thousand observations seem to be required to bring out the results satisfactorily, and the best results are derived from the use of both groups. The principal western maximum occurs six minutes after the lower culmination of the moon, and amounts to 0′.23. The secondary maximum occurs fourteen minutes after the upper culmination, and amounts to 0′.18. The principal minimum occurs at $6^{h.}$ $17^{m.}$ after the lower culmination, the easterly deflection being 0′.22. The secondary minimum at $6^{h.}$ $03^{m.}$ after the upper culmination has a deflection of 0′19. The greatest range is 27″ and the secondary 22″. The epochs of the maxima and minima are found from the formulæ to be at a mean ten minutes after culmina-

tion. The probable error of a single computed value of the lunar declination is $\pm$ 1″.32. The Toronto observations gave $\pm$ 1″.37 from more than twice the number of observations, so that the Philadelphia observations are worthy of every confidence. At Toronto, from the second investigation, embracing about forty-four thousand observations, the western and eastern deflections balanced, giving for the range 38″.3. The Prague observations also confirm the nearly equal deflection (mean) to the west and east. The epochs of the maxima and minima were found from the four roots of the equation $0 = 0.029 \cos (\theta + 295°) + 0.414 \cos (2 \theta + 85°)$ which gave ten minutes as the mean time elapsed between the moon's passing the meridian and the time of maxima of deflection. If we take the four phases into account, the lunar action seems to be retarded ten minutes, which may be termed the *lunar magnetic interval* for the Philadelphia station. At Toronto the intervals are not so regular. The secondary range exists there, and is also a marked feature in the Prague result.

The lunar diurnal variation seems to be subject to an inequality depending on the solar year, for the investigation of which the preceding results were rearranged in two groups, one containing the hourly values for the summer months (April to September), the other the values of the winter months (October to March). For the summer season we have 11,087 observations, and for the winter 10,557.

In the complete memoir the tables of the hourly sums of the lunar variation for the summer and winter seasons are given, and the tabular results are expressed analytically. The curves representing them are shown in the annexed Diagram C.

The characteristic feature of the annual inequality in the lunar diurnal variation is therefore a much smaller amplitude in winter than in summer. Kreil, indeed, inferred from the ten years' series of the Prague observations, that in winter the lunar diurnal variation either disappears, or is entirely concealed by irregular fluctuations, requiring a long series for their elimination. The method of reduction which he employed was, however, less perfect than that now used. The second characteristic of the irregularity consists in the earlier occurrence of the maxima and minima in winter than in summer. The winter curve precedes the summer curve by about one and three quarter hours. Both these features are well expressed in the diagram C. At Toronto the same shifting in the maxima and minima epochs was

noticed, but the other irregularity in the amount of deflection is not exhibited. It seems probable that the Philadelphia results are more typical in form than those either of Prague or Toronto.

It is also apparent that the smaller deflection at the upper culmination in the annual mean, when compared with the deflection at the lower culmination, is entirely produced by the feeble lunar action in winter. The maximum west deflection in summer occurs actually near the upper culmination. At the same season, the maximum east deflection is still retained (as in the annual curves) about six hours after the lower culmination. In the winter season this last-mentioned maximum east deflection is actually smaller of the two. We have

Maximum summer range	$35''.4$,	secondary	$31''.8$
" winter "	25.2,	"	15.6
Difference	10.2,	"	16.2

At Prague the maximum summer range was $44''$.

Next I proceed to examine, whether the phases of the moon, the declination or parallax, have any sensible effect upon the magnetic declination. Dr. Kreil found from a ten years' series of observations at Prague, that there was no specific change in the position of the magnet depending upon the moon's phases and parallax, but that the declination was $6''.8$ greater when the moon was at the greatest northern declination than when at the greatest southern declination. On the contrary, Mr. Brown, from the Makerstown observations, a much shorter series than the one at Prague, inferred that there was a maximum of declination two days after the full moon. He also found a maximum corresponding to the greatest northern declination of the moon, but does not appear to have investigated the effect of distance.

The residuals, which we have been treating, enable us at once to examine these several points.

Beginning with the lunar phases, the daily means for the day of full and new moon, and for two succeeding days, were compared with the monthly mean declination. In case any of the hours were disturbed, the monthly normal for the hour was substituted for the disturbed observation before the mean was taken. If one half or more of the hourly readings were disturbed, the daily mean was altogether omitted. Accidental omissions of hourly observations were supplied by the hourly normal. The half monthly normals were then com-

pared with the half monthly means. In the table of differences thus formed, equal weight is given to the bi-hourly and hourly observations. The daily mean having been subtracted from the month, the positive sign indicates a western deflection and the negative sign an eastern one, as compared with the normal position. The following table contains the result:—

	Sum of deflections.	Number.	Deflection.		
			d.	′	′
Full moon,	+ 11.6	52	+ 0.22	+ 0.10	± 0.07
1st day after,	— 7.1	51	— 0.14	— 0.06	
2nd day after,	— 9.3	48	— 0.19	— 0.08	
New moon,	— 11.5	43	— 0.27	— 0.12	± 0.09
1st day after,	+ 1.5	47	+ 0.03	+ 0.01	
2d day after,	+ 4.4	49	+ 0.09	+ 0.04	

The effect is very small, scarcely much beyond the probable error, but the table indicates that the north end of the magnet is deflected to the westward 0′.1 at the full and as much to the eastward at the change day, the range between full and new moon being 0′.2. A more definite result could hardly be expected from a series of observations extending over but five years.

Treating the subject of the effect of the moon's variation in declination in precisely the same manner, we obtain the following result:—

MEAN DEFLECTION.

One day before,	— 0′.20	from	54	days	of	observations.
At noon's maxima declination,	— 0.10	"	53	"	"	"
One day after,	— 0.09	"	55	"	"	"
Mean,	— 0.13	"	162	"	"	"
One day before,	— 0 04	"	54	"	"	"
At moon's minima declination,	— 0.07	"	52	"	"	"
One day after,	+ 0.14	"	52	"	"	"
Mean,	+ 0.01	"	158	"	"	"

The results do not positively prove a deflection of the magnet, depending on the moon's greatest north and south declination. The amount resulting from the comparisons being of nearly the same magnitude as its probable error.

A similar investigation with respect to the moon's distance from the earth gives the following results: —

	MEAN DEFLECTION.					
One day before,	— 0′.18	from	50	days	of	observations.
At moon's perigee,	— 0.18	"	41	"	"	"
One day after,	— 0.00	"	59	"	"	"
Mean,	— 0.12	"	150	"	"	"
One day before,	— 0.02	"	55	"	"	"
At moon's apogee,	— 0.20	"	53	"	"	"
One day after,	— 0.13	"	47	"	"	"
Mean,	— 0.12	"	155	"	"	"

The difference being of the same order of magnitude as the probable errors, no conclusion as to the effect of distance can be drawn from them.

I propose hereafter to extend the discussion of the moon's effect on terrestrial magnetism to the earth's magnetic force.

III. CHEMISTRY.

1. On the Combustion of Wet Fuel, in the Furnace of Moses Thompson. By Prof. Benjamin Silliman, Jr., of New Haven, Conn.

In all ordinary modes of combustion, it is well known that the use of wet fuel is attended with a very great loss of heat, rendered latent in the conversion of water into steam. As the most perfectly air dried wood still contains about 25 per centum of water, according to the experiments of Rumford, the term *wet fuel* might seem appropriate to all fuels, but mineral coal and charcoal. But technically, this term is restricted to substances like peat and those residual products of the arts which, like spent tan, begasse and dye-stuffs, contain at least one half and often more than half of their weight of water. Until a recent period the attempt to consume these products as sources of heat has been attended with uneconomical results, or total failure. It is the object of this paper to describe a mode of combustion in which by a

modification in the form of the furnace the combustion of wet fuel is not only rendered consistent with the best economical results; but which as it involves chemical reactions never before, it is believed, successfully applied for such purposes, is deserving of particular notice from a scientific as well as from a practical point of view.

It is a well-established fact in chemistry, that the affinity of carbon for oxygen, at high temperatures is so strong, that if oxygen is not present in a free state, any compound containing oxygen, which happens to be present is decomposed, in order to satisfy this affinity. This fact is well illustrated in the familiar case of the Blast Furnace where this affinity is employed to deprive the ores of iron of their oxygen in the process of reduction to metallic iron.

In the first stages of combustion, in wet fuels, the chief products given off are steam from the drying of the wet mass, smoke or volatilized carbon and oxyd of carbon, with, of course, a variable proportion of carbonic acid and carburetted hydrogen. These products in all ordinary furnaces, pass on together into the stack, carrying with them the heat which they have absorbed and rendered latent. The problem presented is then to recover the heat thus locked up and lost, and by the furnace now under consideration *this is accomplished by shutting off almost entirely the access of the outer air and causing the wet fuel to supply its own supporter of combustion drawn from the decomposition of the vapor of water at a high temperature by its reaction with free carbon and the oxyd of carbon.*

The practical solution of this problem was first successfully accomplished by the late Moses Thompson, in 1854. The controversial questions growing out of this invention, are entirely foreign to our present purpose, and in no way affect its practical or scientific value. Suffice it to say, in passing, that we find in this invention another instance of a truth already so often signalized in the history of inventions, that important results are often obtained, of the highest value in promoting material prosperity and the welfare of society, by those who are guided in their search only by the result in view, and not by any exact knowledge of the scientific principles involved.

Mr. Thompson seems to have been inspired with the conviction that if he could bring the products from the combustion of wet fuel together in a place hot enough for the purpose, and from which the atmospheric air was excluded, they would, as he expresses it in his patent, mutually

"*consume each other.*" This notion was realized, and the reaction secured between the elements of water and the carbon of smoke, or the oxyd of carbon in a part of the furnace called by the inventor, the *mixing chamber.*

Wherever that place may be situated, or however constructed, the one essential thing about it is, that it should be a very hot place, and one to which the atmospheric air can have no direct access, until it has passed by, and through the burning fuel. It is in fact a retort or place for combination and reaction, and may be a distinct chamber or flue, or only a recess or enlargement greater or less of the main furnace. Wherever it may be placed, or however built, it must meet the essential conditions of a high temperature, and of atmospheric isolation. In this mixing chamber, then, the important chemical reaction before insisted on, must be set up. The vapor of water is decomposed, furnishing its oxygen to the highly heated carbon to form carbonic acid, while the oxyd of carbon is in like manner exalted to the same condition, and any excess of carbon forms with free hydrogen, marsh gas or light carburetted hydrogen. The vapor of water is thus made to give up not only its constituent elements to form new compounds with oxygen, producing in the change great heat, but a great part of the heat absorbed by the water in becoming steam is also liberated in this change of its physical and chemical condition. The free hydrogen, oxyd of carbon and marsh gas, generated by this reaction among the elements of the wet fuel, pass out of the mixing chamber over the bridge-wall beneath the boiler where, becoming mingled with atmospheric air they burn and wrap the boiler in a sheet of flame, to which source of heat is to be added of course the very high temperature of the mixing chamber from which these gases escape.

Such is the intensity of heat in that portion of the furnace where these reactions take place that only the most solid structures of refractory fire-bricks will endure it, and the color seen throughout that portion of the furnace is of the purest white.

In view of the facts already stated, it is easy to understand why it is that when the reactions described are once set up, the admission of a free current of atmospheric air should immediately check the energy of the combustion and soon result in total suspension of the peculiar energy of this furnace. The air containing only one fifth part of its bulk of oxygen gas, the active agent in combustion, the access of so

large a proportion of cold air — four fifths of which are not only indifferent but positively prejudicial from the quantity of heat it absorbs, — it happens that the temperature of the mixing chamber is rapidly reduced below the point at which carbon can decompose vapor of water, and the instant that point is reached the arrival of fresh supplies of steam completes the decline of energy and the furnace commences forthwith to belch forth from its stack dense volumes of smoke and watery vapor. When in proper action not a particle of smoke is visible from the stack of a furnace in which wet fuel is burning, and what is more remarkable, the reactions are so evenly balanced that no wreaths of watery vapor are observed, while in the earlier stages of combustion before the proper temperature in the mixing chamber is reached, both these products are seen in great abundance.

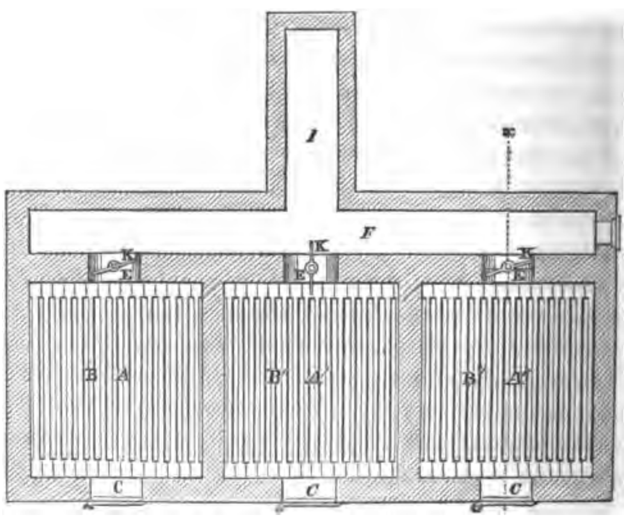
DESCRIPTION OF THE FURNACES.

1st. Furnace for combustion of wet tan, sawdust, &c.

Fig. 1, is a horizontal section of a furnace constructed according to the specifications of Thompson's first patent (issued April 10th, 1855).

Fig. 2, is a vertical section of the same in the line $x\ y$, of figure 1.

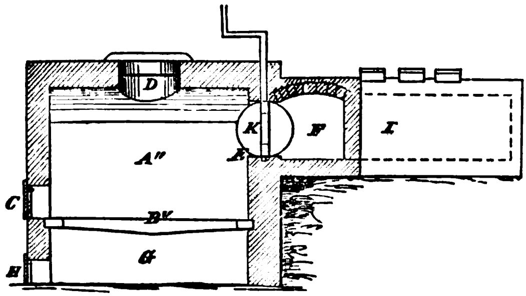
FIG. 1.



Similar letters indicate corresponding parts in both figures.

The furnace shown in these figures has three square or oblong fire chambers, *A*, *A′*, *A″*, side by side, experience having shown that not less than three compartments are required to secure the best results in the practical working of the furnace, although in some cases two may

FIG. 2.



suffice, but frequently more than three are desirable. The fire chambers are furnished with grate bottoms *B*, *B′*, *B″*, of fire bricks, and are arched at top. Each chamber has a door *C* in front, for lighting and tending the fire. The opening is seldom used after the furnace is once set in action. The wet fuel is supplied through the opening *D* at top. *E* is an opening at the back of each chamber leading to the flue *F*, or the mixing chamber. This opening may be provided with a damper *K*, which should be of fire-clay; if of iron the intense heat soon destroys it. Each chamber has a separate ash-pit *G* with its opening *H*. This although called an ash-pit serves a most important purpose in the economy of the furnace as a receptacle for the burning coals which constantly fall into it from the lower part of the wet mass above, as will be more particularly explained beyond.

If the furnace is used for generating steam the best place for the boiler is over the flue *I*. The inventor remarks in his first patent that the current from the mixing chamber in passing to the place of use, in the case of burning wet tan, or other very wet fuel, should descend or pass under a bridge to the place of use equal to about one half of the depth of the burning chamber between the grate and the crown, then rise to the place of use. In case of dry, or nearly dry fuel, such as green wood and sawdust, the current should rise immediately after leaving the burning chamber to the place of use.

The mode of conducting the operation of the furnace is as follows:

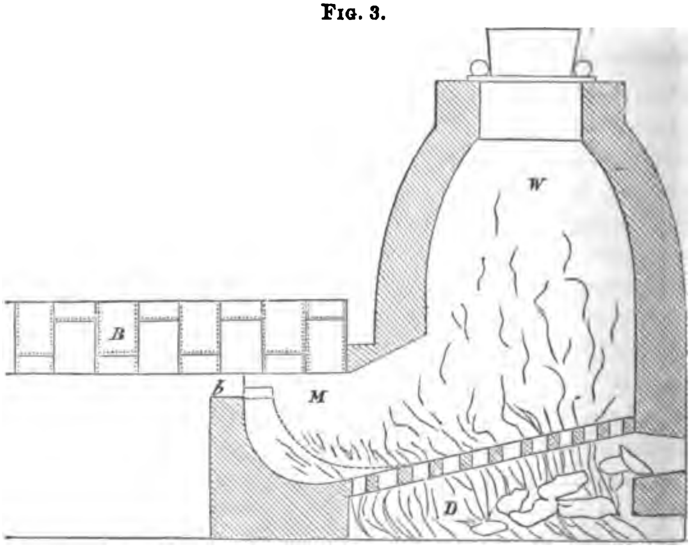
fires being lighted in all the fire chambers with dry fuel, and the masonry heated to a high degree, two of the three chambers A A' are fed with wet fuel and have their ash pits closed. The other fire chamber is kept in action by dry fuel (its ash pit door being proportionally open) until the process of combustion sets in over the surface of the pile of wet fuel resting on the grates of the other furnaces. As soon as this is the case, wet fuel is added by degrees to the third fire chamber, the ash pit door being at the same time closed. If things have been properly managed so far, the process will now continue by the addition of new portions of wet fuel to each furnace in succession, or alternately. The temperature of the mixing chamber F is now seen to be of the most perfect whiteness, and not a visible particle of smoke issues from the stack.

Before discussing this process more in detail, let us first consider the inventor's description of his furnace as designed more particularly for the consumption of begasse or crushed cane stalks.

2. *Furnace for Combustion of Wet Cane Begasse.*

Fig. 3 is a sectional side view, the interior and exterior form of the

FIG. 3.



furnace, and its several parts according to the specifications of Thompson's patent of Dec. 15, 1857.

Fig. 4 is a front sectional view of the same, showing the combination of two double furnaces.

FIG. 4.

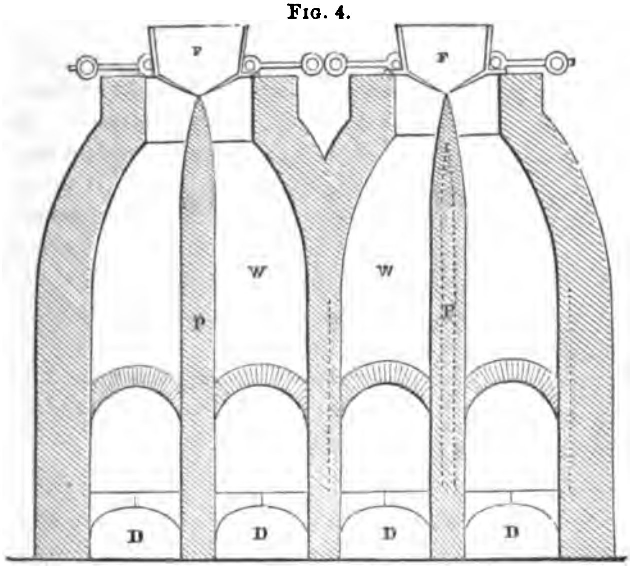
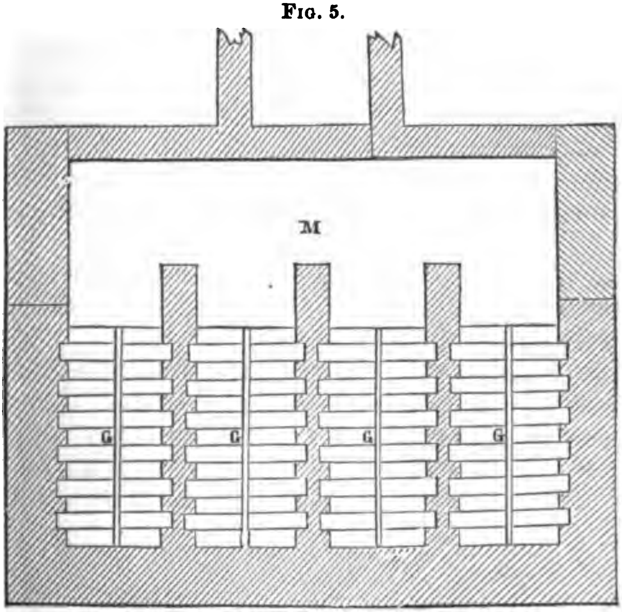


Fig. 5 is a horizontal view of the grate and its relation to the mixing chamber *M* and flue *F*.

FIG. 5.



Here let the inventor speak for himself in the language of the patent last named.

"I build two furnaces side by side, each nearly square in its horizontal section. Towards the top I draw in the wall in such a manner as to form a kind of dome with a sufficient opening at top to feed the begasse. The outer walls of these furnaces should be from twenty-four to thirty inches thick, and built with a special view to rendering them non-conducting, the wall near the top, and the partition between the two furnaces may be thinner. In each furnace chamber there should be a partition of fire brick extending across it from front to back and rising nearly to the top, dividing it into two nearly equal parts. The whole interior of the furnace should be of fire brick. The main chamber of each furnace should be divided into two parts — upper and lower — by a fire brick grate about one fifth the height of the furnace above the hearth, the back end of the grate being a little lower than the front. The bottom of the lower chamber may be a grate with an ash pit, but a hearth is much better.

"In each furnace at the front, on each side of the central partition and immediately under the front end of the grate should be doors for feeding wood or other dry fuel, and directly under these doors at the hearth of the lower chamber should be draught openings capable of adjustment to support combustion in the lower chamber.

"Extending across the back of both furnaces, and opening into both by flues, is a mixing chamber, into which all the gases from both furnaces enter in a highly heated state, and mix and consume each other on their way to the boiler and stack. This chamber should be about one half the capacity of all the fire chambers, and it should extend down about as low as the back end of the grate. The flue through which the products of combustion pass out of this chamber and under the boiler should be in section about one square foot to forty cubic feet of mixing chamber.

"The feed openings at the top of the furnaces should be closed by doors which open inward by the weight of the feed, but are self-closing, and do not yield to pressure from within.

"The sides of the interior of the upper or wet fuel chamber or drying chamber of the furnace, except the front and back, are corrugated up and down, as also the sides of the central walls or partitions, as shown by the dotted lines in Fig. 4, the corrugations extending down

to the grate; these corrugations are for the purpose of allowing the heat to radiate upwards from the fire chamber for heating the masonry, and the wet charge, while the gases or vapors driven out of the wet charge by the heat are allowed to descend to the fire chamber or the mixing chamber. If the surfaces of this masonry were smooth, the bagasse would lie against them in such a manner as to obstruct the upward radiation of the heat, and the downward passage of the vapors.

"These corrugations are unnecessary in burning tan and sawdust.

"The spaces between the grate bars for burning bagasse should be about six inches wide for the finest grinding, and twenty inches for the coarsest, and should vary between these widths according to the fineness of grinding, but for sawdust and tan much less, say from one inch to three fourths of an inch. The grate should be made of fire brick.

"The operation of my furnace is as follows: A hot fire of dry fuel is kindled in the lower or fire chambers of the furnaces, and after it has been continued till the masonry is well heated, the chamber above the grate is fed with the begasse or other wet fuel. This hot fire in the fire chamber, especially towards the front of it under the principal mass of the wet fuel, must be preserved throughout the operation. The heat from the masonry and the fire chamber will be communicated to the wet fuel which will cause steam and other gases to issue from it and mix with the intensely hot gases of combustion from the fire chamber, and in a short time the mixing chamber will present intense combustion and heat, the dampers of the fire chambers being partially closed. The lower part of the wet charge will by degrees become dry and charred, and will fall through the grate prepared as above unto the fire chamber, and supply or nearly supply the place of other dry fuel in preserving the fire in this chamber, and the wet fuel being from time to time supplied, will furnish in a highly heated state aqueous vapors, which descending through the corrugations and otherwise into the fire chamber and mixing chamber, will be decomposed, furnishing much oxygen to the fire, and supply the oxygen necessary to combustion of all the combustible gases issuing from the chamber. If, by accident, the fire in the lower part of the furnace should predominate, the draught should be diminished, and more wet fuel added, and, if by accident, the fire in the fire chamber should become too much cooled down, the draught should be let on, and any deficiency of dry fuel should be

supplied to the fire chamber. Under proper management, little or no dry fuel need be fed to the fire chamber after the operation is fairly commenced, the charred matter falling through the open gate will supply its place; and the caloric thus produced by the combustion of wet fuel, will be vastly greater than from the same quantity by measure of the same fuel when dry. In the fire chamber, and in the mixing chamber, under intense heat, the carbonaceous gases will decompose the steam from the wet fuel, and effect complete combustion.

"When the operation is fairly commenced, if the water in the wet charge amounts to say fifty per cent. by weight of the fuel, the dampers of the fire chamber should be nearly or quite closed to exclude the air; vapor from the wet charge will then descend through the corrugations and otherwise into the fire chambers and support the combustion therein, while other portions of the vapor will enter the mixing chamber and complete the combustion there. If the fuel, however, contains much smaller quantities of water, more air in proportion should be admitted at the damper, the object being to admit no more air than will supply the deficiency of the vapor.

"In the drawings, *D* represents the chambers for the dry fuel, *W* those for the wet, *M* the mixing chamber, the dotted line *m* in Fig. 3 limits it for the wettest bagasse, P the partition, F the feed openings for the wet fuel with their doors, B the boiler, *b* the bridge. Little if any of the boiler should extend over the mixing chamber. If any considerable portion of the mixing chamber is covered by the boiler its cooling influence will prevent the decomposition of the vapor and defeat the object of my invention. Great care should be observed in giving proper dimensions to the mixing chamber, for the perfection of the combustion and the efficiency of the furnace depend greatly upon it. The principal object of this chamber is to give the combustible carbonaceous gases from the fire, and the aqueous gases from the mass of wet fuel an opportunity of mingling together in such a manner and under such circumstances that the aqueous vapor will be decomposed by the carbonaceous gases, and its oxygen given out to complete the combustion of the carbon, without the introduction of air into the mixing chamber, thus saving the caloric previously communicated to the wet charge, while drying it and charring its lower portions, and avoiding the cooling influences of cold air. This can take place effectually only in the presence of a high degree of heat and in the absence of a supply of free oxygen. If this chamber be too small to receive these

gases as fast as the furnace is able to produce them the operation will of course be choked and impeded. If the chamber is larger than can be kept densely filled with these gases, of course atmospheric air will be found there at the commencement, and will continue to find its way into the chamber, and while atmospheric air is present, the carbonaceous gases will take their oxygen from that source principally instead of decomposing the steam, and the heat in the chamber will be much diminished and the large quantity of nitrogen (⅘), contained in the air, which is neither combustible nor a supporter of combustion, will at once greatly increase the volume of gases to be sent forward to the stack and proportionably decrease its temperature; and when the chamber becomes very large the cooling influences become so great that combustion will immediately cease, and smoke mingled with steam, oxygen, and nitrogen, will go forward, thus wasting the fuel and imparting only a faint degree of heat to the boiler.

"I have therefore fixed the size of the mixing chamber by many careful experiments — and that given above will produce the desired effect with wet bagasse. For dryer fuels furnishing less vapor, the mixing chamber should be proportionably increased in size to supply the deficiency with air, and to effect complete combustion. Rules more precise would be inconsistent with the nature of the subject.

"A large and hot fire should always be preserved in the fire chamber below the grate, and directly under the charge of wet fuel, for the purpose of driving the vapor out of it and charring its lower portion — and the grate is left much more open than in furnaces for burning dry fuel of the same size, for the purpose of allowing the charred portions of the wet charge to fall through to supply fuel for this fire as fast as it becomes fit for that purpose, thus consuming the mass with little or no expenditure of other fuel.

"What I claim as my improvement in furnaces for burning bagasse and other fuels too wet to be conveniently burned in the usual way and well known ways, is:

"First, the combination of two chambers, the one above the other, and separated by a grate, the lower one for the combustion of any known dry carbonaceous fuel, and the upper one in immediate proximity therewith to receive heat therefrom for heating and drying the charge of wet fuel, with a mixing chamber, into which both continuously and simultaneously discharge their gases before reaching the thing to be heated, for mingling and mutual combustion.

"I also claim in combination with said fire chamber and wet fuel chamber, or drying chamber, making the grate upon which the wet charge rests sufficiently open to allow the lower portion of the wet charge as it becomes dried and charred to fall through into the fire chamber and keep a hot fire therein, supplying the place of other dry fuel, while the uncharred portions of the wet fuel are properly supported by the grate till dried as described.

"I also claim placing the mixing chamber for combustion in substantially the same position described relatively to the fire, and the wet charge, so that the products of combustion from the dry fuel may pass along the lower part of the wet charge, drying and charring it on their way to the mixing chamber, and reach it without being in any considerable degree obstructed or cooled by the wet charge substantially as shown.

"I wish it distinctly understood that I make no claim to any of the parts or combination above specified except in their application to the preparation and combustion of wet fuels."

It will be observed that in this mode of combustion the wet fuel is subject to a constant process of distillation by the fire in the ash-pit. The products of this distillation react on each other in the mixing chamber in the manner already described, while at the same time a portion of watery vapor is decomposed in the ash-pit.

Theoretically no more heat can be generated in this mode of combustion than is consumed in the transformation of water into steam and the conversion of fixed into volatile products. But it is by no means a matter of indifference whether the oxygen requisite for complete combustion is drawn from the atmosphere or is derived from the decomposition of water by carbon and its oxyd. In the former case, not only is there a great loss of heat carried away by the inefficient nitrogen of the air, but the diluted oxygen can never produce so intense a heat with the carbon as is the result of the reaction of the nascent oxygen with that element. Although Mr. Thompson was no chemist, he did not fail with his natural acumen to perceive this advantage, and in his earliest patent he remarks: "After ample experiments I have discovered that any results that can be produced by the use of dry fuel are inferior (to those obtained from my process) in proportion to the quantity used, and that results like mine can only be obtained by the use of wet fuel . . . fed into an intensely heated chamber: under such circumstances the water in the fuel in presence of the carbonaceous sub-

stances in the furnace will be decomposed, giving its oxygen to the carbonaceous matter, dispensing with a draft and its cooling and wasteful influence, and rendering the combustion so perfect that no smoke is visible."

Although this mode of combustion of wet fuel is now in use on many sugar plantations in Louisiana, and in some tanneries of Pennsylvania and New York, no notice of it has, so far as I am aware, appeared in the scientific journals. I am not without personal experience of its operation on a large scale, having in 1857 enjoyed the opportunity of studying carefully the management of one of Thompson's furnaces in three compartments (similar to Figs. 1 and 2), built for the combustion of wet peat. That fuel contained over seventy-five per cent. of its whole weight of water, and was too wet for the best results. But with the use of one fourth part of dry wood, even this extremely wet and otherwise valueless fuel was rendered efficient, three cords (of 128 cubic feet) of wet peat and one cord of dry wood doing the work of four cords of dry wood in driving a steam boiler.

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2. Further Remarks on Numerical Relations between Equivalents. By M. Carey Lea, of Philadelphia, Penn.

In papers on this subject published in the January and May numbers of Silliman's American Journal for the present year, I endeavored to show that a large number of so-called elements could be arranged in seriated groups, the members in each series differing from each other by a common quantity, in most cases the number 44 or one approximating to it. I endeavored to show that not only were these groups natural groups, but that the chemical properties of the members of each group corresponded in many cases with their position in it. These observations seemed to favor the view at present gradually gaining ground, that those bodies which we have as yet failed to decompose, we have not proved to be elementary.

An interesting and elaborate paper by Gustav Tschermak, published in the Proceedings of the Academy of Science of Vienna, and extracted in an abridged form in Knop's Centralblatt (July 4, 1860), on

the subject of the law of volumes of liquid chemical compounds, affords a support to the views above expressed, from a new source. The author therein shows that many of the substances usually classed as elements comport themselves in the physical properties exhibited by their combinations as compound bodies, and that it is possible from these physical properties to determine (hypothetically) the number of "physical" or absolute atoms which he supposes to be contained in a chemical atom of such body or pseudo-element. He endeavors to show that it is possible to calculate the specific gravity of a liquid from its atomic weight and the number of simple (chemical) atoms in its compound molecule, as data, but that the results lead to the immediate inference that each chemical atom contains, with few exceptions, several physical atoms.

For particulars of his theory I must refer to the original paper, but some of his results are as follows:—

	Physical atoms to each chemical atom.*
O (O = 16)	2
S (S = 32)	4
F	2
Cl	4
N	2
P	4
As	5
Sb	6

If now we arrange the first six of these substances in parallel series we shall find

	Atomic weight.		Physical atoms.
Sulphur	32		4
Oxygen	16		2
Difference	16	Difference	2
Chlorine	35.5		4
Fluorine	19		2
Difference	16.5	Difference	2
Phosphorus	31		4
Nitrogen	14		2
Difference	17	Difference	2

* These numbers are taken from the table of mean numbers, p. 508 of Centralblatt, and are those subsequently used by the author for determining the "physical atomic weights" $= \frac{m}{n}$.

Thus a common difference in each case amounting to 16–17 corresponds with a difference of two of the physical atoms into which Tschermak divides the chemical atoms.

If now we put $O = 2o$, $Cl = 2cl$, $P = 2p$, the approximate difference between S and O, Cl and F, etc., $(16–17) = 2\Delta'$, the difference (48) between S and $Se = \Delta''$ and the difference (44–45) between the terms of the nitrogen series $= \Delta$, we can express the whole of three important series in terms of these six quantities, so that at one and the same time both the numerical value of the atomic weights and the number of Tschermak's physical atoms shall be correctly expressed.

	Symbols.	At. weights.	Physical atoms.
A. Oxygen group.			
Oxygen	o_2	16	2
Sulphur	$o_2 \Delta'_2$	32	4
Selenium	$o_2 \Delta'_2 \Delta''$	80	5
Tellurium	$o_2 \Delta'_2 \Delta''_2$	128	6
B. Chlorine group.			
Fluorine	fl_2	19	2
Chlorine	$fl_2 \Delta'_2$	35.5	4
Bromine	$fl_2 \Delta'_2 \Delta$	80	5
Iodine	$fl_2 \Delta'_2 \Delta_2$	127	6
C. Nitrogen group.			
Nitrogen	n_2	14	2
Phosphorus	$n_2 \Delta'_2$	31	4
Arsenic	$n_2 \Delta'_2 \Delta$	75	5
Antimony	$n_2 \Delta'_2 \Delta_2$	120.3	6

In which table the number of radicals by which the chemical atom of each body is expressed, corresponds with the number of Tschermak's physical atoms, while their numerical value is equal to the atomic weight of the body.

Thus tellurium $o_2 \Delta'_2 \Delta''$ would have two each of three radicals, in all six, agreeing with the number of physical atoms assigned to it, while their value $2 \times 8 + 2 \times 8 + 2 \times 48 = 128$, at. wt. of tellurium.

These observations of Tschermak, taken in connection with the numerical relations which exist between atomic weights, give rise to very interesting results, and if the conclusions which he arrives at from his experiments should be confirmed, they cannot fail to exercise a very important influence on the progress of chemical science.

3. Note on Sources of Error in the Employment of Picric Acid to detect the Presence of Potash. By M. Carey Lea, of Philadelphia, Penn.

Picric acid enjoys a high reputation as a test for potash. Employed in its alcoholic solution, or as soda or ammonia salt, sometimes as magnesia salt, it gives with potash solutions a dense yellow crystalline precipitate. If the solution containing potash be very dilute, the precipitate may not make its appearance till after some hours' repose, and it then forms long delicate needles.

Rose remarks that this reagent "is even more sensitive than the solution of chlorid of platinum." In his summing up, he observes, that of the various reagents, chlorid of platinum, tartaric acid, picric acid, perchloric acid, sulphate of alumina, and hydrofluosilicic acid, the latter is insufficient to distinguish between potash and soda; and that the chlorid of platinum and sulphate of alumina give the same reactions with ammonia as with potash.* No qualification is made with respect to the certainty of the indications afforded by picric acid. Plattner makes the same observation as to the greater sensitiveness of picric acid compared with bichlorid of platinum and equally without qualification as to its reliability.†

I therefore believe that the remarkable insolubility of otherwise soluble alkaline picrates in alkaline solutions has not been before pointed out. If an alcoholic solution of picric acid be poured into a solution of carbonate of soda, it occasions an immediate dense yellow precipitate, not to be distinguished in appearance from a precipitate of picrate of potash, and liable to be mistaken for it with the greatest facility. The picrate of soda formed in the case just mentioned is the most soluble of all the alkaline picrates, and *à priori* we should not expect to find it precipitated under these circumstances: an aqueous solution of picrate of soda added to one of carbonate of soda acts in precisely the same manner.

To ascertain if these reactions were extended to various compounds of picric acid, examinations were made which gave with the following results:

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* Rose, Handbuch der Analytischen Chemie, 1r Band, 6–10 S.

† Plattner, die Probirkunst mit dem Lothrohre, 3d Ed. p. 178.

Alcoholic solution of picric acid added to
solution of carbonate of ammonia, gave an immediate dense yellow crystalline precipitate.
" carbonate of soda gave not so instantaneous a precipitate, but one which after standing became even more dense.

Aqueous solutions of picrate of soda gave with

solution of carbonate ammonia,	abundant precipitates.
sulphate of ammonia,	
carbonate of soda,	
phosphate of soda,	slight precipitate.

Aqueous solution of picrate of ammonia gave with

solution of carbonate of ammonia,	abundant precipitates.
" sulphate of ammonia,	
" carbonate of soda,	
" phosphate of soda,	none.

Aqueous solution of picrate of magnesia gave with

solution of carbonate of soda,	abundant precipitates.
" carbonate of ammonia,	

When the alkaline solutions to be tested are otherwise than very dilute, there is absolutely no difference in the appearance of the precipitates. When the amount of precipitate is small, the following differences may be observed:

The potash precipitate forms longer needles, and when these are formed only after standing, or when the precipitate is redissolved by heat and allowed to crystallize by slow cooling, they exhibit a beautiful play of red and green colors.

The soda salt is of a lighter and brighter yellow than either the potash or ammonia salt. Its needles are also shorter than those of either of the other compounds, and when the precipitate has been redissolved by heat, and allowed to crystallize by slow cooling, they tend to agglomerate themselves together in spherical masses. This is a very marked character, and when exhibited, is unmistakable, but is not always shown.

The ammonia salt when slowly crystallized in quantities is very distinct in appearance from the potash salt, but when thrown down in small portions, even by slow crystallization, cannot be distinguished from it with certainty. The prisms are flatter, but these differences are not apparent in very slender needles. The play of colors which the ammonia

salt exhibits to a less degree than the potash, disappears entirely in the soda salt.

The above reactions indicate that extreme caution must be used in employing picric acid as a test for potash. The precipitates above described do not redissolve in any excess of the picrate, or of picric acid; nor is it to be supposed that concentrated alkaline solutions are necessary to cause them — on the contrary, in many cases, very dilute ones are sufficient. To determine within what limits such reactions are produced, the following trials were made:

A. Dilution of the alkaline solution. Limits obtained:

(I). With carbonate of soda,

A solution containing five per cent. anhydrous carbonate of soda gave
with alcoholic solution of picric acid, an immediate abundant precipitate, in ten minutes the tube could be inverted without spilling the contents.
with solution picrate of soda, an immediate precipitate becoming in the course of an hour a very dense one.

A solution containing $2\frac{1}{2}$ per cent. carbonate of soda gave
with alcoholic picric acid, a slight precipitate increasing very much by standing, so that after eighteen hours the tube could be inverted without spilling the contents.
with picrate of soda, none.

A solution containing one per cent. carbonate of soda gave
with alcoholic picric acid, slight precipitate after eighteen hours' repose.
with picrate of soda, none.

(II). With sulphate of ammonia.

A solution containing five per cent. anhydrous sulphate ammonia gave
with alcoholic picric acid, immediate dense precipitate.
with picrate soda, immediate dense precipitate.

solution containing $2\frac{1}{2}$ per cent. sulphate of ammonia gave
with alcoholic picric acid, immediate dense precipitate.

solution containing one per cent. sulphate of ammonia gave
with alcoholic picric acid, immediate dense precipitate.
" solution picrate soda, immediate slight precipitate. The solution on standing gave a beautiful crystallization of long needles with bright play of colors, exactly resembling the reaction of potash salts.
" picrate of magnesia, by long standing, a very faint precipitate.
" picrate of ammonia, none.

solution containing $\frac{1}{250}$ anhydrous sulphate of ammonia gave
with alcoholic picric acid, after a short interval, an abundant precipitate.

solution containing $\frac{1}{500}$ anhydrous sulphate of ammonia gave
with alcoholic picric acid, after a short interval, a considerable precipitate.

solution containing $\frac{1}{1000}$ anhydrous sulphate of ammonia gave
with alcoholic picric acid, no precipitate, even after twenty-four hours' repose.

B. Limits obtained with respect to indications of diluted solutions of alkaline picrate.

A solution of picrate of ammonia in 200 parts water gave —with an equal volume of strong solution of carbonate of ammonia, an immediate precipitate of small yellow needles — in ten minutes, a considerable quantity settled at the bottom of the vessel.

A solution of same salt in 400 water gave with an equal volume of solution carbonate of ammonia, signs of a precipitate in a few minutes, and after some hours an appreciable quantity settled at the bottom of the vessel.

With one part picrate of ammonia in 800 water no precipitate was produced by solution of carbonate of ammonia, even after twenty-four hours' repose.

The conclusion to be drawn from these results is,

That alcoholic solution of picric acid or aqueous solution of picrate of soda will produce a precipitate in almost any alkaline solution, whether of soda, ammonia, or potash, except under circumstances of great dilution, especially if allowed to repose for twenty-four hours.

That picrate of ammonia and picrate of magnesia give the same results, but in a less degree.

That picric acid is therefore wholly unreliable as a test for potash; the results obtained being such as would tend altogether to mislead those who are not extremely familiar with the appearance of the precipitates, and that in some cases the results are so deceptive that even eyes most familiar with these reactions might be deceived; for example, in the result obtained above by testing a solution containing one per cent. sulphate of ammonia with solution of picrate of soda. In this case a crystallization of picrate of ammonia was obtained perfectly simulating that of the potash salt.

Picric acid is in fact a better test for soda than for potash, because with a soda solution it gives a precipitate which redissolved by heat generally (but not always) gives a characteristic spherically radiated, bright canary yellow crystallization, whereas the precipitate obtained from a potash solution can never be positively distinguished by its appearance from that afforded by an ammonia solution, and we have just seen that a solution containing $\frac{1}{300}$ of sulphate of ammonia or even less, is capable of producing such a precipitate.

4. On the Production of Ethylamine by Reactions of the Oxy-Ethers. By M. Carey Lea, of Philadelphia, Penn.

While engaged in making a series of experiments on this subject, I met with the paper of Juncadella * and the observations of De Clermont † on the same subject. Finding that it has less novelty than I supposed, I merely offer here one or two of the results which I have obtained.

Nitrate of ethyl $C_4 H_5 O, NO_5$ heated in sealed tubes with chloride of mercurammonium $\left.\begin{matrix} H_3 \\ Hg \end{matrix}\right\} N\,Cl$ for many hours in the water bath did not appear to react upon it. Kept for some time in a boiling saturated solution of chlorid of calcium, the tubes, although extremely thick green glass combustion-tube of small calibre was used, exploded with great violence, shattering the vessel in which they were contained, although they had been wrapped in strong cloth.

Nitrate of ethyl heated in a sealed tube with chlorid of zinc-ammonium $\left.\begin{matrix} H_3 \\ Zn \end{matrix}\right\} N\,Cl$ in the water bath, does not appear to act upon it.

Nitrate of ethyl heated in the water bath in a sealed tube with carbamate of ammonia $NH_4 O, NH_2 C_2 O_3$ dissolves the salt. On cooling, radiated crystals form. The contents of the tube evaporated to dryness with excess of chlorhydric acid, and then exhausted with ether, to which a few drops of strong alcohol have been added, yielded a solution which gave a chamois colored precipitate with bichlorid of platinum, consisting of chloroplatinate of ethylamine.

·1185 gm. substance gave ·0465 metallic platinum corresponding to 39·25 per cent.; theory requires 39·29.

The product was but small. Probably portions remained undissolved by the ether. No doubt portions of di-ethylamine and tri-ethylamine are also formed in the above reaction, in the same manner as in those of the halogen ethers with ammonia.

* Rép. de Chimie pure, Tome 1, 273. † Ibid. 274.

5. On the Optical Properties of the Picrate of Manganese. By M. Carey Lea, of Philadelphia, Penn.

Brewster and Haidinger have described a remarkable property possessed by certain crystalline surfaces, of reflecting, besides the ray normally polarized in the plane of incidence and reflection, another ray, polarized perpendicularly to that plane, and differing from the former in being colored, a property rendered more conspicuous by the fact that the color of the ray so polarized abnormally is either complementary to, or at least quite distinct from the color of the crystal itself.

I find that this property is possessed to a remarkable degree by the picrate of manganese. This salt crystallizes in large and beautiful transparent right rhombic prisms, sometimes amber-yellow, sometimes aurora-red, exhibiting generally the combination of principal prism, and macrodiagonal, brachydiagonal, and principal end planes. In describing this substance in a paper on picric acid and the picrates,* I mentioned that in a great number of specimens examined, no planes except those parallel with or perpendicular to the principal axis had been met with. Since then I have obtained in several crystallizations specimens exhibiting a brachydiagonal doma, but this appears to be rather unusual.

The optical properties of this salt are very interesting. It exhibits a beautiful dichroism. If the crystal be viewed by light transmitted in the direction of its principal axis, it appears of a pale straw color, in any other direction, rich aurora-red in some specimens, in others salmon color. A doubly refracting achromatized prism gives images of these two colors, except the light be transmitted along the principal axis of the crystal of picrate, in which case both are pale straw color.

But it also possesses in a high degree the property of reflecting two oppositely polarized beams, and the great size of the crystals in which it may readily be obtained, renders it peculiarly fitted for optical examination. If one of these crystals be viewed by reflected light while it

* American Journal of Science, etc., November, 1858.

is held with its principal axis lying in the plane of incidence and reflection, the reflected light is found to be not pure white, but to have a purple shade. Examined with a rhombohedron or an achromatized prism of Iceland spar, having its principal axis in the plane of incidence and reflection, the ordinary image is white as usual, while the extraordinary is of a fine purple color, the phenomenon having the greatest distinctness when the light is incident at the angle of maximum polarization.

The experiment may be varied, and the purple light beautifully seen, without the use of a doubly refracting prism, by allowing only light polarized perpendicularly to the plane of incidence to fall on the crystal; in this case the surface of the crystal appears rich deep purple, no white light reaching the eye.

This property is not possessed by all the planes of the crystal, but is limited to the principal prism and brachydiagonal and macrodiagonal end planes, in other words to the planes parallel with the principal axis of the crystal. The brachydiagonal doma and 0 *P* planes do not possess it. Nor is it exhibited by the first-mentioned planes, when the crystal is turned with its prismatic axis at right angles to the plane of incidence.

All specimens of picrate of manganese do not possess this property to an equal extent. The crystals vary considerably in color, and those which are full red exhibit it more strongly than the amber colored. Picric acid boiled with aqueous solution of cyanhydroferric acid and saturated with carbonate of manganese gives crystals of a rich deep color, which exhibit the purple polarized beam particularly well.

These properties are not possessed by the manganese salt alone, but also by the picrates of potash and ammonia (especially when crystallized by very slow spontaneous evaporation in prisms of sufficient size), and the picrates of cadmium and peroxyd of iron, — with this difference, however, that while the prismatic axis of the crystal in the case of the cadmium and manganese salts must be in the plane of incidence, in the alkaline salts it must be perpendicular to that plane. As they all crystallize in the right rhombic system, it is probable that either the alkaline salts on the one hand, or the manganese and cadmium on the other, are prismatically elongated in the direction of a secondary axis.

It is convenient that distinct phenomena should have distinct names, and none appears to have been assigned to this. Brewster speaks of it as a "property of light," and Haidinger uses the word "Schiller" for it. The terms dichroism, trichroism, and pleiochroism are limited to properties of transmitted light. I therefore suggest for that here in question the name *catachroism*, using the preposition *κατα* in the same sense as in the word *κατοπτρίζω*, to reflect (as a polished surface), applying it to express the property of reflecting two beams, one normally polarized in the plane of incidence, and the other polarized in a plane perpendicular to it.

The chromatic properties exhibited by the picrates of ammonia and potash are very remarkable in their variety. Their crystals possess, —

1st. The well-known play of red and green light. If a little very dilute solution of pure picrate of potash be spontaneously evaporated in a hemispherical porcelain basin, so as to form a network of extremely slender needles, and these be viewed by gas light, the play of colors is singularly brilliant.

2d. Dichroism. When by spontaneous evaporation of large quantities of solution of potash, or better, of ammonia salt, transparent prisms of $\frac{1}{12}$ to $\frac{1}{10}$ inch diameter are obtained; these, viewed with a doubly refracting prism by transmitted light, give two images, one pale straw color, and the other deep brownish red.

3d. The above described property of catachroism, or reflection in the plane of incidence of oppositely polarized beams.

B. NATURAL HISTORY.

GEOLOGY AND PALEONTOLOGY.

1. GEOLOGY OF THE ISLAND OF AQUIDNECK. By CHARLES H. HITCHCOCK, of Amherst, Mass.

HAVING been requested, in behalf of the city of Newport,* to prepare a geological map of Aquidneck, or the island of Rhode Island, for the use of the members of the Association at this meeting, I would, in this communication, make a few observations respecting the variety, age, position, and origin of the different rocks found upon the island.

There are nine different varieties of rocks represented upon the map. They are, 1. Granite and protogine; 2. The lowest rocks upon the island, consisting of talcoid grits, often largely composed of grains of sand and pebbles; 3. Metamorphic slates; 4. Conglomerates; 5. The true coal measures; 6. Beds of dolomitic or magnesian limestone; 7. Seams of serpentine; 8. Alluvium and drift; and, 9. Beds of coal. Most of these rocks were produced in the Carboniferous age. Perhaps the lowest slates may be older, and the granite may have been formed during the Permian or later periods. I will describe these rocks in the order of their formation, beginning with the oldest.

1. *Lower schists and conglomerates.*—The only place upon the island where these are developed, is upon the promontory which is terminated by Sachuest Point. Lithologically, the rocks are whetstone talcose schists, grits, and soft slates, often containing many pebbles of quartz and grains, so that they might be termed talcoid conglomerates. We noticed a few pebbles of red jasper among the constituents. This

* Mr. Charles E. Hammett, Jr., of Newport, authorized the execution of the survey and the publication of the map at his own expense, in the month of June. Afterwards the authorities of the city purchased of him 300 copies of the map for the members of the Association.

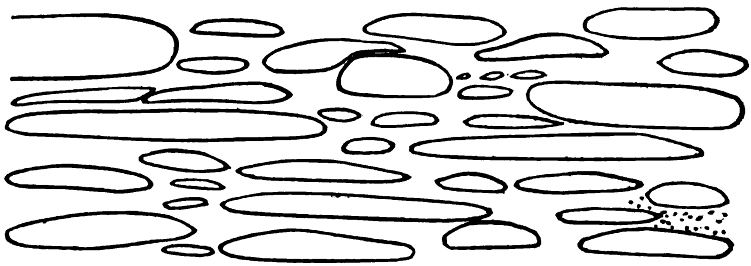
fact distinguishes this group of rocks from the overlying conglomerates. These schists have a direction of N. 20° E. and dip to the west 45°, towards the coarse conglomerates at Purgatory. Cleavage and jointed planes are abundant in these rocks upon Sachuest Point. The age of these strata is probably older than the Carboniferous series, though there is little difference in their external aspect from that of the slates west of Purgatory. There are no beds of coal or other fossils in them, and this exposure of them is an outlier isolated by denudation from the same strata in Little Compton. Their thickness upon Sachuest Point cannot be less than 1,000 feet. These rocks have been more or less metamorphosed, so that some of the pebbles appear like crystals of quartz and felspar in a crystalline schist.

2. *The conglomerate at Purgatory.*—This group of strata consists of one of the most remarkable conglomerates ever described, associated occasionally with belts of slates, as at Taggart's Ferry. The range is more than a mile wide, showing itself most southerly below Purgatory, composing the Hanging Rocks and Paradise, and probably underlying the drift as far north as Sandy Point in Portsmouth. Cormorant Rock in the ocean is also a part of the range. There are three ridges of this conglomerate at its southern part,—Purgatory being located in the western, and the Hanging Rocks composing the eastern one. The middle ridge is a hard, gritty rock.

The conglomerate at Purgatory may be considered as the type of this rock. There is a ridge of naked conglomerate upon the east side of the small promontory lying between Easton's and Sachuest beaches. This ridge runs N. 30° E. An examination of the rock itself shows it to be a coarse conglomerate, composed of elongated and flattened pebbles, from the size of grains of sand to boulders nearly twelve feet in length, cemented by talcose schist or sandstone,—the cement being exceedingly meagre in amount. The pebbles are mostly of a fine-grained quartz rock, often jointed in the larger nodules. Sometimes decomposition shows this quartz rock to be a sandstone, and often it passes into mica schist and gneiss. One large pebble was beautifully specked with crystals of felspar. Very rare is it to find any other variety of rock among the pebbles. One kind that we noticed was a soft micaceous schist, having scattered through it small shining crystals of a micaceous mineral resembling adamsite. Scattered through the conglomerate everywhere are an immense number of minute crystals of mag-

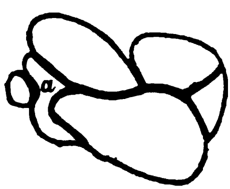
netite. These pebbles are so arranged that their longest diameters are uniformly parallel to one another, in the direction N. 30° E., the same as the strata. The large drawing (reduced in Fig. *a*), represents an

FIG. *a*.



exact copy of the pebbles upon a surface about six by two feet of this conglomerate. The pebbles are seen to be parallel to one another, and most of them wonderfully elongated. This is a fair sample of the general appearance of these rocks. Not merely are these pebbles elongated, but they are also flattened, as may be seen wherever joints have made actual sections across the strata. Still another distortion of the original shapes of these pebbles is seen in a cross section (Fig. *b*). The pebbles, as if compressed together, have accommodated themselves to those positions which afforded the greatest freedom, — a round pebble often fits into a sort of a cap like a ball and socket joint, both the ball and the socket being separate pebbles. One pebble, (*a*) in the figure, seems to have been forced into a shape not at all symmetrical. These cases are very common.

FIG. *b*.



There is another feature of this conglomerate no less peculiar than the shapes of the pebbles. The ridge is divided by perpendicular fissures or *joints* running east and west and parallel to one another, from a few inches to many feet apart. Sometimes the rock between two of these perpendicular planes is removed, as in the excavation called *Purgatory*. This chasm runs about east and west at right angles to the coast; is from eight to ten feet wide, thirty-six feet deep on one side, and forty-four on the opposite side. It is a number of rods in length. To the south of this there is a smaller purgatory in process of formation. Dr. C. T. Jackson says that this chasm was once filled up by a trap dyke, of which he found a "small portion remaining in the

south [west] end of the rent, to attest its former presence;" and that the dyke "has been worn away by decomposition and by the mechanical action of the sea below." The southern extremities of the different ranges of conglomerate are terminated by these joints abruptly, so that steep walls are presented to view, where the bisection of the pebbles is seen in a striking manner.

An examination of a single joint shows three things; 1st. The surfaces of the rocks upon both sides of the joint are perfectly smooth, as if they had been glazed rather than polished by friction. 2d. The pebbles are cut in two, or the larger ones into several pieces by these joints; insomuch that the parts of the pebble may be taken out of the ledge and placed together end to end to show that they make but one pebble. The joints appear just as if a Titan had slashed through the ledges with an immense scimetar. 3d. The rock has not been moved since the joints were produced. Each pebble fits exactly without the slightest displacement, to its corresponding part upon the opposite side of the fissure, unless it be an occasional mass disturbed by an injected vein, or has been moved recently by frost or the waves. In some places these joints are occupied by veins of white quartz.

These pebbles must have been distorted and elongated since their deposition and subsequent consolidation as conglomerate. No one ever sees such unnatural shapes or such a parallel arrangement of boulders along any shore at the present day, and there is no reason why they should have been thus deposited in Palæozoic times. If the pebbles have been elongated and curved since their deposition, then the whole rock must have been in a plastic or semi-plastic state after its consolidation. Our theory in regard to the origin of the forms is, then, that owing to an elevated temperature — perhaps at the bottom of the ocean — the conglomerate became soft. Elevating agencies plicated the strata, and the tension produced by the curving of the strata, and the force of the lateral pressure, elongated and flattened the pebbles. The finer particles, or the cement, by the same process, were altered into talcose schist. This process is essentially that of the metamorphism of all the azoic rocks of New England. It but just commenced at this locality, so that we learn from these facts, not only the origin of the distortions, but the mode of metamorphism in general.*

* This subject will be very fully developed by my father in the forthcoming

Probably when the rock was plastic the joints were produced by feeble galvanic currents permeating the mass in consequence of the chemical decompositions and recompositions caused by the conversion of sandstone into schist. In other words, the process may be the same as the origin of joints in schists or of cleavage in slates.

The position of the strata of this conglomerate is very interesting because it throws light upon the origin of the distorted pebbles. The strata are a succession of small folds, — there being more than six anticlinals in the whole belt of rock. The best place to examine these convolutions is on the shore of the point, proceeding southwest from Purgatory. At Purgatory the strata are rather obscure, as they are composed principally of large pebbles, — yet they seem to dip to the west in general at a high angle. An isolated ledge in the ocean east of the range dips to the east at a less angle. Passing to the end of the conglomerate, the observer will pass over four or five similar folds, every one of them having the eastern slope moderate, and the western slope much steeper. We have never seen a locality where the folds are so numerous over so short a distance. Without examining the two ranges of hills of this conglomerate carefully, we have concluded that they are separate folds. Certainly there is a fold to the east of Paradise, which may be traced along the east coast of Middletown, between Sachuest Beach and Taggart's Ferry.

These folds correspond beautifully to the normal curves of the Middle States described by Professor Rogers, as well as to anticlinal axes, more or less all over the globe. It is an important fact also, that wherever the strata have been crumpled and bent or fractured, the greatest amount of heat has manifested itself, either in igneous eruptions, or quiet metamorphism of rocks. So here, the metamorphic action has been most powerful where the folds are numerous.

There is a difficulty in comparing this conglomerate with synchronous deposits elsewhere, because of the uncertainty with respect to the age of the rocks beneath, and the absence of the usual Carboniferous limestone. It may correspond to the millstone grit of England, or more probably to the sub-carboniferous conglomerates of New York

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Report on the Geology of Vermont. Localities of conglomerate, even more altered than the example at Purgatory, are found in the towns of Wallingford and Plymouth in that State.

and Pennsylvania. Yet in none of those localities are there conglomerates containing such immense or distorted pebbles. The parent rock from which the pebbles of quartz in this belt of conglomerate were derived is unknown. There is a large amount of quartz rock west of this coal basin in north-eastern Rhode Island and the adjacent parts of Massachusetts, which may have been the source of some of them.* But more probably the original rock is now entirely worn away, or covered up by some newer formation, or, still more probably, metamorphosed into some azoic rock.

There is more history in one of these pebbles than one not familiar with the science of the earth is apt to imagine. This pebble is a constituent of a coarse conglomerate. Of course, then, the pebble was once a part of a rock somewhere else; from whence it has been torn and dragged unwillingly along to its new resting-place by ruthless, unsparing agencies. But this pebble is itself a sandstone. Therefore before it was torn from this original ledge, it existed on the shore as sand unconsolidated. Whence the grains of sand? They were derived from many different ledges, and have been worn and crushed ages before the sand was consolidated. And hence we have the history of these earlier ledges to trace out. But it is impossible to say whether these earlier ledges were stratified or igneous; whether they formed the original crust of the globe, or whether the cycle of formation and removal has been undergone at other times. But it is certain that the exact constituents of this pebble have been parts of ledges in three different geological periods.

Similar deposits in Eastern Massachusetts. — Along the borders of the Massachusetts and Rhode Island coal basin this conglomerate is conspicuous. Upon the east side it is found in Tiverton, R. I., Fall River, Somerset, Swanzey, Dighton, and Berkley, Mass. Upon the west side of the basin it is found in Warwick, Cranston, Providence, R. I., and further to the northeast in Massachusetts. Jackson says that instances of this coarse conglomerate may be seen all along the road from Providence to Roxbury, Mass.

* Professor W. B. Rogers showed specimens of pebbles taken from this conglomerate, as it appears at Fall River, containing fossils of Lower Silurian age. This shows us the age of the rocks from which the conglomerate was derived. We hope the paper will be published in the Proceedings of the meeting.

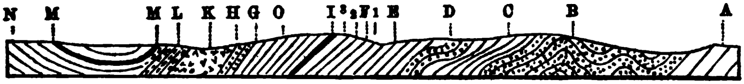
It appears to be certain that this conglomerate overlies a gritty schist or sandstone, and is itself overlaid by the various slates of the coal measures. Perhaps this order of succession may assist us in comparing this basin with two small Palæozoic basins in Eastern Massachusetts. The most distinct basin is in the vicinity of Boston, separated from the Newport basin by syenite. The order of succession is the following: Underlying syenite, Braintree slate, containing Paradoxides Harlani, grits, sandstones, and a few beds of conglomerate, clay slate, conglomerate closely resembling that at Purgatory, as in Roxbury, Dorchester, Brighton, etc., and upon the top of the whole, the old gray wacke and clay slates. As there are no fossils in the upper part of the Boston basin, it may be presumptuous to suggest that the Roxbury conglomerate is equivalent to the Purgatory conglomerate, and the overlying rocks to the lower part of the coal measures.

In Hingham, upon an eastern spur of this basin, the order is similar. First above the syenite is a slate which is lithologically the same as the Braintree slate, then the conglomerate, and lastly the overlying slates. This Hingham conglomerate contains some immense pebbles of syenite; also pieces of green and red porphyries and other unstratified rocks,— an interesting fact to prove that the syenite is pre-silurian.

There is a very small Palæozoic basin in Essex county, in the towns of Rowley and Newbury, along Parker River. The order of rocks is the same as at Hingham,—a narrow strip of slate, sometimes entirely disappearing, next the syenite, then coarse conglomerate, and above all the slates and schists. In this basin the rocks dip to the north universally, and consequently they appear to dip under the syenite. But there is a suggestion worthy of note, which may render probable an inversion of the strata upon the north side of the basin. At Hingham, as suggested by Mr. T. T. Bouvé, the conglomerate is gradually merged at certain localities into red jasper,—as if the pebbles had been fused,—that is, the conglomerate rock is metamorphosed into red jasper and porphyry. Similar appearances occur at the south part of the Parker River basin, where we found it difficult at first to decide whether a particular rock was conglomerate or red jasper. At the north side of the basin there is a thick bed of jasper, and we would suggest that it is the result of the alteration of the conglomerate; more easily altered because of its unnatural position. On this supposition, the same rock is found on both sides of the basin. Perhaps the sug-

gestion that this conglomerate is equivalent to the conglomerate at Purgatory will be considered too hypothetical for belief, — yet to us it seems probable.

FIG. *c*.



Section at the south end of Aquidneck Island.

Fig. *c* illustrates the position of the different rocks in the south part of Aquidneck Island. The measurements of the strata are all upon the route of this section. The east end is at A, at Sachuest Point, and the rocks are the lower grits and schists. Purgatory is at B, and two plications of the first conglomerate are exhibited in the sketch as a sample of the six or seven that may be found there. The schists and slates No. 3 are at C. At D the second conglomerate shows itself, terminating westward at the east end of Easton's or the "first beach" E. At F the coal measures appear in a high bluff. At I is a bed of coal. The figures 1, 2, and 3 denote localities of fossil plants, to be mentioned hereafter. The city of Newport is at O. At G is the third conglomerate, which occurs at Newport Neck, and is therefore a little out of the proper line of the section. The granite and protogine are represented at K. L represents silicious slate, with a few seams of serpentine. M M represents the beds of dolomite cropping out in the two islands of dolomite and near Fort Adams. Fort Adams is at N, where may be seen also the upper part of the coal measures dipping east.

3. *Schists and slates west of Purgatory.* — The conglomerates at Purgatory are very much contorted as they pass beneath the slates to the west. The transition from conglomerates to schists is gradual; in fact, there are several beds of conglomerate intercalated at intervals in the schists. For quite a number of feet it is impossible to estimate the thickness of these slates, because the small flexures are so numerous, and the cleavage is sometimes perplexing. The section which is given below was measured along the shore of the point south of Purgatory, and will represent both the variety and the thickness of the different layers. The enumeration commences with the lowest stratum, and proceeds upwards:

Soft talcose schists, and a few seams of clay slate	46 ft. 8 in.
Talcose and novaculite schists, dip 10° W.	30 ft. 3 in.
Conglomerate	8 ft.
Schists	12 ft.
Whetstone schists and talcose grits, corrugated by the action of the waves	55 ft. 6 in.
Conglomerate	9 ft.
Talcose schists and grits, N. 20° E., dip 30° W.	19 ft.
Grits	37 ft. 6 in.
Novaculite schists (a tolerably good hone-stone)	21 ft.
Whetstone schists, dip 34° W.	17 ft.
Grits, sandstones, black shales	4 ft. 3 in.
Fine conglomerate and grits, dip 31° W.	9 ft. 3 in.
Whetstone schists and gray grits, dip 24° W.	18 ft.
Conglomerate	4 ft.
Mica schist	10 ft.
Conglomerate containing an unusual amount of talcose cement	15 ft.
Gray grit, dip 38° W.	15 ft.
Measures concealed	48 ft.
Mica schist, dip 30° W.	6 ft.
Soft black slate, dip 30° W.	3 ft.
Light-colored mica schist, containing small grains of sand or small pebbles, N. 20° E., dip 62° W.	84 ft. 8 in.
The total thickness of these strata is	473 ft. 1 in.

4. *Second Conglomerate.* — Alluvial deposits cover so much of the island, that only a small part of the second deposit of conglomerate is exposed to view. Upon the map the coal measures are represented as surrounding the conglomerate; but this is done because the rock was covered with soil. We found an excellent exposure of the different layers, however, along the coast, southeast of Easton's Beach, and directly succeeding the slates No. 3 on the west. This conglomerate is composed of the same materials as that at Purgatory, but the pebbles are much smaller, and only in a few cases are distorted or elongated; and beds of grit and schists are more common. The following section gives the particulars, commencing at the base as before: —

Conglomerate, dip 62° W.	14 ft. thick.
Brown grit	18 ft.
Conglomerate and sandstone	3 ft. 6 in.
Conglomerate	9 ft.
Black grit	3 ft.
Conglomerate	17 ft.
Dark gray grit	2 ft. 6 in.
Conglomerate	2 ft. 3 in.

Grit and conglomerate	4 ft.
Black slate	3 ft. 4 in.
Grit and conglomerate	8 ft.
Compact black slate, N. 20° E., dip 62° W.	30 ft.
Conglomerate	51 ft.
Conglomerate and grit	12 ft.
Conglomerate	54 ft.
Black compact grit, interlaced with delicate white quartz threads	1 ft.
Gray grit	10 ft.
Conglomerate, N. 25° E., dip 70° W.	6 ft.
Black grit and seams of sandstone	15 ft.
Conglomerate	9 ft. 3 in.
Very hard black slate, N. 25° E., dip 42° W.	9 ft.
Hard black slate, dip 48° W.	26 ft. 8 in.
Conglomerate	10 ft.
Measures concealed	7 ft.
Conglomerate	13 ft.
Measures concealed	21 ft.
Conglomerate	6 ft.
Measures concealed	48 ft. 7 in.
Grit and conglomerate	15 ft.
Measures concealed	20 ft.
Dark grit	3 ft.
Coarse conglomerate	12 ft.

A synclinal axis is now reached, and the last 155 feet 7 inches of the section are repeated, dipping to the east. This brings us to the east end of Easton's Beach, where it is impossible to examine the succeeding strata as they are deeply seated beneath the sand. The conglomerate may be seen stretching to the northeast upon the west side of a long hill, — hence it cannot connect with the conglomerate upon Miantonomah Hill, to be described presently as an upper bed, and the third conglomerate. The total thickness of the second conglomerate is 464 feet and one inch.

We suppose that this conglomerate turns again and dips beneath the slates on the west side of the beach. When last seen the dip is only 5° E., and the undulation seems to be a local affair. Estimating the length of the beach at 2,690 feet, and the westerly dip of the underlying rocks at twenty-five degrees, the thickness of these concealed measures is 920 feet. They probably belong to the coal measures.

5. *Coal Measures.* — We now have arrived at the rocks which compose the greater part of the island. The chief varieties of rock are the

following: 1. A dark colored slate, or slaty clay, often much indurated, and more or less charged with carbon. This is generally in the vicinity of beds of coal; 2. Beds of anthracite and plumbago; 3. A hard slate, usually of a talcose green color, but embracing also every other shade of green, which does not appear to be talcose, micaceous, or argillaceous; sometimes it is micaceous. This is the most common variety of rock in the coal measures; 4. Grits and sandstones, both compact and easily decomposing; 5. Conglomerates made up of pebbles of slate. This is quite distinct in lithological character from the conglomerates already described.

The following is the succession of strata from the west end of Easton's Beach to the third conglomerate in the south part of Newport:— Carbonaceous slate, N. 10° E., dip 30° W. ten feet.

Among the debris fallen from this and the two or three following beds are fragments containing the following species of plants. We suppose that they originated from the preceding stratum. They are: *Asterophyllites sublævis*, Lsqx.; *Pecopteris arborescens*, Brgt.; *Annularia sphenophylloides*, Brgt.; *Aphlebia;* n. sp. *Pecopteris* (*Alethopteris*) *unita*, Brgt.; *P. affinis*, Brgt. (now for the first time found in North America); *Sphenophyllum emarginatum*, Brgt.; *Sphenophyllum schlotheimii*, Sternb.; *Sphenopteris*, n. sp.; and *Annularia fertilis*, Sternb. This is the locality No. 1, in Fig. *c*.

Conglomerate made of pebbles of argillo-micaceous slate	16 ft.
Carbonaceous slate and conglomerate	3 ft. 6 in.
Carbonaceous slate, dip 40° W.	19 ft.
Conglomerate from 3 to 9 feet thick. Average	6 ft.
Black friable carbonaceous slate, dip 48° W.	17 ft. 4 in.
In this slate there are numerous perpendicular veins of white quartz.	
Conglomerate of slate pebbles	11 ft.
Gray sandstone	11 ft.
Black friable carbonaceous slate	10 ft. 6 in.
Conglomerate	7 ft.
Gray sandstone	7 ft.
Gray sandstone and black slates	12 ft.
Micaceous slates	20 ft.
Hard mica schist, N. 10° E., dip 30° W.	16 ft.
Shales and sandstone	8 ft.
Hard mica schist	12 ft.
Talcose schist	10 ft.
Conglomerate full of white quartz veins, thinning out southerly	16 ft.

Black shales and sandstones	12 ft.
Buhrstone	3 in.
Black shales	6 in.
Black sandstone, very evenly bedded, dip 37° W.	15 ft.
Greenish sandstone	7 ft. 3 in.
Black slate, dip 18° W.	23 ft.
Sandstone, corrugated by the surf	12 ft.
These rocks are permeated by cleavage planes, dipping west.	
Sandstone and schist	28 ft.
Green shales	6 ft.
Coarse conglomerate	17 ft.
Sandstone	1 ft. 6 in.
Black slate	8 ft.
Shales and sandstones, dipping 25° W.	4 ft.
Gray sandstone	4 ft. 6 in.
Conglomerate	12 ft.
Gray sandstone	8 ft.
Slates	5 ft. 6 in.
Conglomerate	8 ft.
Green slate, very friable	10 ft.
Green slate, more compact	3 ft.
Soft talcose schist	8 ft.
Slates	11 ft.
Shales, very friable	5 ft. 7 in.
Aluminous shales	3 ft.
Conglomerate	11 ft.
Aluminous shales	8 ft.
Black shales	9 ft.

Conglomerate, thirty feet, in which there have been slides, so that beds of black slate have been removed from their original position, and are now situated like injected veins which have been cut off by other injections. There are many small contortions of strata at this point in the section.

Black slates	9 ft. 6 in.
Conglomerate	6 ft.
Glazed black slates	3 ft. 6 in.
Green sandstone	14 ft.
Light green talcose schist	15 ft. 9 in.
Conglomerate	30 ft.

There is a fold here, the rock dips 62° E., running N. 10° E. This is north of Ochre Point. There are several small faults in the strata just described, sometimes showing a dislocation amounting to twenty feet. Passing over the fold the first rock above the conglomerate is

Altered conglomerate	9 ft.
Whetstone talcose schist	14 ft.
Conglomerate containing elongated pebbles, somewhat resembling the Purgatory conglomerate	23 ft.
Schist and conglomerate	4 ft.
Soft talcose schist, dipping 40° W.	8 ft.
Conglomerate, Ochre Pt.	8 ft.
Slates	4 ft.

Another small fold appears here.

Above the repeated rocks are slates, sandstones, and conglomerates, dip 16° W. . . . . 25 ft. thick

Next there is another long wave or fold of the strata, at the west end of which there is a bed or vein of quartz 18 feet thick, running N. 20° W., and dipping 20° S. W.

The rocks in this last wave correspond in lithological character with the rocks overlying and displaced by the granite at Sheep Point. Supposing the horizon to be the same, we continue the section from near Sheep Point. The granite would naturally act along the line of strike, and hence it is that the strata are disturbed about Ochre Point.

Rocks mostly concealed, about 50 feet, containing *Pecopteris nervosa*, Brgt.

Shales	25 ft.
Shales containing *Alethopteris Pluckneti*, Brgt., and *Neuropteris tenuifolia*, Brgt.	25 ft.

This is locality No. 2 in Fig. *c*.

Shales and anthracite	2 ft.
Carbonaceous shales	10 ft.
Shales and anthracite	4 ft.
Sandstone	6 ft.
Gray shales	15 ft.
Sandstone, about	18 ft.
Carbonaceous slates	20 ft.
Impure anthracite	1 ft.
Carbonaceous shales	19 ft.
Shales	28 ft.
Conglomerate	4 ft.
Shales	15 ft.
Gray sandstone	6 ft.
Very friable black shales	32 ft.
Conglomerate	3 ft.
Black sandstone, running N. 20° E.	9 ft.
Anthracite	1 ft.
Fine sandstone	2 ft. 6 in.

Shale containing the following plants (two feet thick): *Asterophyllites sublævis*, Lsqx.; *Pecopteris arborescens*, Brgt.; *Annularia fertilis*, Sternb.; *Neuropteris*, n. sp., allied to *N. Grangeri*, Brgt.; *Lepidodendron*, *Pecopteris* (*Alethopteris*) *unita*, Brgt.; *P. dentata*, Brgt.; *P. cyathea* (?) Brgt.; *Sphenopteris elegans*, Brgt.; *Annularia sphenophylloides*, Brgt.; *Sphenophyllum schlotheimii*, Sternb.; *Cyclopteris*, n. sp.; *Lepidodendron* (*Sagenaria*) *dichotomum*, Sternb.; *Sphenopteris intermedia*, Lsqx.; *Pecopteris arguta*, Brgt.; *P. oreopteridis*, Brgt.; and two indeterminable species of *Pecopteris*. This is locality No. 3 in Fig. *c*.

Anthracite	6 in.
Black shales, sometimes gritty	20 ft.
Anthracite	1 ft.
Shales and slates	6 ft.

Further to the north, and probably overlying the preceding, is the following order of strata:

Carbonaceous shales containing numerous bunches of anthracite	16 ft.
Mica schist	9 ft.
Carbonaceous shales, containing three inches of anthracite	24 ft.
Sandstone, containing nodules	8 ft.
Carbonaceous shales with anthracite	19 ft.
Dark mica schist	13 ft.
Black slates, dip 24° W.	20 ft.

This point in the section brings us to the grounds of Mr. Barreda. From thence westward (or upwards) the rocks are mostly covered by drift, so that we could not measure them bed by bed. The total thickness of the rocks upon the section just enumerated, between Easton's Beach and Barreda's, is 1,091 feet and 2 inches.

Upon Pope street in Newport there is an abandoned quarry of micaceous schist. The strata run N. 20° E., and dip 28° W. They are somewhat contorted. A similar rock is exposed upon the north side of Levin street. Estimating the distance from the last point of measurement to the third conglomerate as 3,155 feet, and the average dip of the strata as 30° W., the thickness of the strata would be 1,577 feet, making the total thickness of the strata between the second and third conglomerates to be 3,588 feet and 2 inches.

In the west part of Newport the strata dip east, as they are upon the west side of a synclinal axis. The synclinal line lies along the junction of the metamorphic with the unaltered slates. Upon Price's

Neck, the rock is an indurated bluish slate, whose cleavage planes are unusually far apart. It is overlaid by a similar rock, but containing some decomposing substance, rendering the color grey. To the west of this rock the slate is very finely divided by cleavage planes, and is quite soft and friable. Half a mile west of Price's Neck, along the shore, there is a dyke (?) of a decomposing greenish rock, overlaid by a vein of white quartz from six to ten inches thick. The surface of the quartz is fluted. In the rock west of this there are innumerable nodules of white saccharoid carbonate of lime, running N. 33° W., and dipping 22° E. Some of the ledges abound in cavities from which the lime has been removed by the dissolving action of the waves.

The rocks upon Castle Hill are green and dark brown shales often somewhat indurated, running N. 42° E., and dipping 28° S. E. Between Castle Hill and Fort Adams similar rocks occur, dipping at first 20° and then 15° S. E. Within the fortress beneath the parade ground the position is the same.

In the north part of Newport these greenish slates appear at Fort Green, Malbone Garden, Rose Island, Gull Rock, the south end of Coaster's Harbor Island, and Bishop's Island. In Middletown, most of the surface is covered with alluvium; but the usual strata of the coal measures were noticed in several places; as near an ancient fortification north of Gleason's Pond, north of Miantonomah Hill, and near the geographical centre of the town. Ledges are more abundant in Portsmouth. In the southwest part of the town, a small stream has worn through strata of mica schist and sandstones, producing a picturesque gorge in what is called Lawton's Valley. Between this gorge and the Aquidneck mine ledges of slates and sandstones are common, having quite variable positions, but with a general easterly dip. But all the ledges on the east side of the town dip to the west, thus forming a synclinal axis. At Black Point there are black carbonaceous shales, slates, and fine-grained sandstones, running N. 30° E., dipping 29° W., and permeated by cleavage planes dipping 62° W.

Butts Hill is the site of ancient fortifications, and is in the trough of the synclinal, as the strata dip towards each other on the opposite sides of the hill. The rocks are the usual coal measures of the island. The coal formation does not extend to the extreme north part of the town, for ledges of granite show themselves there, and the junction of the two rocks is obscured by alluvium.

Beds of Coal.—The only bed of coal in the whole of this basin in Massachusetts and Rhode Island that is mined, is in Portsmouth, at the Aquidneck mine.* This is in the north-west part of the town, and has been worked for many years. The strike of the strata and the bed is somewhat east of north, and the dip of the bed now wrought is from 28° to 35° south-easterly, the dip being greatest at the surface. There are three beds of coal, of which chiefly the middle one has been worked, which averages from two and a half to three feet in thickness—the other beds being generally but a few inches thick. The succession of strata in an ascending order is this: sandstone, carbonaceous and clay slates, five feet; the three beds of coal, separated by narrow seams, carbonaceous slates, five feet, and overlying the whole, strata of sandstone.

From Capt. William Cobb, one of the proprietors of the mine, I learn that upon the west side of the synclinal between the Bay and Butts Hill there are *eleven* different beds of coal, all of which are covered with soil. Aquidneck mine is situated upon the eighth bed in the order of ascent from the Bay. Accompanying each bed are one or more "leaders" or small seams of coal. The three beds at the mine Capt. Cobb considers as one bed with two leaders. The three upper beds are about two hundred feet distant from one another topographically.

The thickness of the coal bed which is worked varies from one to twenty feet in consequence of plication of the strata. There are undulations in the beds, and these bunches of coal are accumulated where the pressure was least. These foldings do not run either perpendicular or parallel to the direction of the strata, but obliquely to it. Hence, when a thick mass of coal is reached, it passes into the earth between the strata in an oblique direction, and the axes of all the other convolutions will run parallel to it. We examined the direction of some of these folds, and found them to run N. 62° E., and N. and S. In these cases the force was from the east or southeast. In other parts of the mine the direction is different—indicating a force from the N. E. or S. W. Hence we suppose that the forces operating upon this bed have been exceedingly complicated. The phenomena are such as to

* This is now called the Mount Hope mine.

make it appear as if the strata had been crowded into a tight place, so that really the forces of disturbance came from every quarter. Yet there are no extensive faults. A few small ones appeared, but none greater than four or five feet.

In consequence of these pressures, the coal has been much crushed and broken, so that when dug out it crumbles and produces a great deal of fine coal. The coal is heavier than the anthracite from Pennsylvania, and contains a larger proportion of carbon.

The Mt. Hope mine has been worked successfully for a number of years. It is now seven hundred feet deep, and there is a large number of gangways and openings, branching out at different levels. The mouth of the mine is not more than twenty feet above the ocean.

Upon the opposite side of the island is Case's mine, the opening of which is now entirely closed up. Dr. Jackson states that the strata here run N. N. E., S. S. W., dipping to the W. N. W. 15° to 20°; and that there are three beds of coal, one of them being thirteen feet thick.

As these two beds of coal (Mount Hope and Case's mine) are upon opposite sides of a synclinal axis, and dip towards each other, the question arises whether they are not outcrops of the same bed upon the two edges of the basin? Dr. Jackson estimates the amount of coal in Portsmouth upon the supposition that they are the same bed. But we are not satisfied that the beds are the same. For the synclinal line is on the top of Butts Hill, which is 5,172 feet from Mount Hope mine, and 2,828 feet from Case's mine. Moreover, the dip of the former mine is greater than that of the latter. Hence, unless there has been a disturbance in the strata of great extent, it is impossible for these two beds to belong to the same stratum. The Mount Hope bed should reappear east of Child's Wharf in the strait, and Case's bed should crop out on the west side of Butts Hill.*

* For want of time we did not traverse the region between these two beds; and without digging a deep trench through the soil, we apprehend it would be difficult to decide which one of the eleven beds of the Mount Hope series corresponds to the one at Case's mine. The latter mine, with the adjoining rocks, is entirely covered up with soil, so that it is impossible to decide the question by the aid of the peculiar vegetation associated with the coals. We suspect from one or two previous observations of geologists, which in their present form do not harmonize, that there are *two* synclinals instead of one, — or one synclinal and the west side of

The Mount Hope bed has been traced northerly nearly to Bristol Ferry. There is said to be a bed of plumbago half a mile south of Bristol Ferry, which must be upon a different layer from the Mount Hope bed. Probably both sets of beds will hereafter be traced around the north end of Portsmouth, so as to unite. The soil in that vicinity is thoroughly charged with carbonaceous matter, as if from the disintegration of coal. The alluvium forms quite a thick covering, so that it is impossible without cutting trenches to know the position of the coal.

Some small coal beds have been found in other parts of the island. On Quaker Hill, in Portsmouth, a small coal bed was struck a few years ago, but was not worked. Between this hill and Newport the rocks are concealed, but we have reason to believe that this bed upon the east side of the synclinal axis is continuous through the island. At least coal occurs in two localities in Newport, one upon Green Lane in the north part of the city, and the other upon Newport Neck, upon the estate of Mr. Francis Barreda. The latter consists of several small beds of impure anthracite, associated with coal plants, and presenting their edges to the sea. They were first opened by the British during the Revolutionary war, but were not found to be profitable.

The general structure of the coal measures upon Aquidneck Island is that of a synclinal axis. The greater part of its west edge is worn away, but in the north part of Portsmouth, and the south-west part of Newport below Fort Adams, and on the islands in Newport Harbor, enough of the easterly dip remains to show the structure.

For the age of the coal beds we refer to another paper read at this meeting, and published in this volume, entitled, *Synchronism of Coal Beds in New England with those in other North American Coal Basins.*

6. *The third Conglomerate.*—There are only two deposits of this bed upon the island open to view, and these probably are connected together beneath the soil and water. The most northern of them is at Miantonomah Hill and Coaster's Harbor Island. Miantonomah Hill is capped with a conglomerate, with an occasional seam of slaty sandstone. The strata are nearly horizontal, some dipping a few degrees

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another. For one observation is, "the rocks at Case's mine dip W. N. W. 15°." The other, "the rocks at Case's mine dip 35° south-easterly." Obviously two different localities were in mind, yet not widely separated from each other.

east, and others a few degrees west. The pebbles are mostly quartz, cemented by sandstone, but of much less size than those at Purgatory. The largest pebble is three feet in diameter, and their average length is about two inches. None of them are distorted, but we noticed a few of the peculiar joints cutting through them at right angles, as at Purgatory.

There is another ledge of coarse conglomerate between the top of the hill and the harbor, but nothing of special interest could be learned from it. There is a fine development of this conglomerate upon Coaster's Harbor Island, upon which the almshouse is situated. Most of the rocks upon the island are purely conglomerate; and some of the ledges are a mere collection of pebbles with scarcely enough of a cementing substance to hold them together. None of them are distorted or elongated. Upon the west side of the island the strata are plicated and a beautiful little anticlinal axis is displayed, exhibiting the conglomerate gracefully folding over strata of green slate. It is not certain that these strata of slate are other than constituent parts of the conglomerate, as at the south end of the island the conglomerate distinctly runs under a great mass of the slate.

The most interesting fact observed upon the island was the discovery of small masses of anthracite among the pebbles, thus confirming the observed stratigraphical position of the conglomerate; that is, superimposed upon the coal measures. From its position in reference to beds of coal it may not improbably be the equivalent of the *Mahoning sandstone* of the West. Its thickness we have estimated to be at least fifty feet.

The other locality of this conglomerate is in the south part of Newport, lying nearly in conjunction with the granite. We found it on the east side of Almy Pond in two places. In the most northerly locality the pebbles were not large, and were intermixed with carbonaceous shales. Further south the pebbles were very large and have been distorted into the most uncouth shapes, — very much more than the pebbles at Purgatory. This is explained by its proximity to the granite. The thickness of the whole deposit may have been greater than our estimate, because in one case the upper part may have been removed by erosion, and in the other by becoming metamorphosed into granite.

7. *Metamorphic Rocks.* — Between the metamorphic slates and the coal measures, there is a mass of granite which must have been formed

subsequently to the deposition of the metamorphic rocks, though not subsequent to their alteration. Hence we rank the metamorphic slates as the older of the two.

These metamorphic rocks consist of silicious slate, chert, and jasper. There are two patches of these rocks, both in immediate proximity to granite. They do not differ from the common rocks of the coal measures in mineral composition, but they do in hardness and want of divisional planes. A series of specimens may be obtained illustrating every variety of compactness from jasper or silicious slate to ordinary clay slate or sandstone, — and the change from the original character decreases from the granite outwards. But only those parts of the slate which overlie the granite are affected, unless, perhaps, part of the smaller bed of altered rocks.

The metamorphic rocks resist decomposition greatly, and it is comparatively easy to trace their limits by noticing where the ledges are prominent, and where very little soil has accumulated. The south-west part of Newport, for this reason, is a perfect paradise of ledges.

The largest mass of silicious slate lies between the city of Newport and Fort Adams, and is about three fourths of a mile wide, and over a mile in length. The other locality is between Rough Point and Mr. Barreda's estate. The belt runs transversely across the strata, is towards half a mile in length, and 1,584 feet wide according to the measurement of Dr. Jackson.

Serpentine. — Among these altered rocks near Fort Adams there are seams of serpentine, barely separated from the granite by a belt of the flinty slate. At its eastern extremity it seems to be stratified. It is compact, very hard, and may easily be mistaken for greenstone, as it is of a very dark color.

Dolomitic Limestone. — Near Fort Adams, upon the shore of the harbor, are two beds of dolomitic limestone, running N. 50° E., and dipping S. E. about 40°. One of them is forty-five feet wide, the other fifteen feet. The limestone is variegated, being tinged with red, green, brown, or buff color. Occasionally the limestone is brecciated, and numerous ramifying veins of quartz traverse the whole, checking it in various ways.

There are two islands in the harbor entirely composed of this rock; the larger 210 feet long and ninety feet wide, the smaller about half as large. No planes of stratification were found, but there was a sys-

tem of joints somewhat resembling strata, dipping 49° W. This limestone has been burned, and produces a valuable water cement.

We suppose that these two islands of limestone correspond to the two beds upon the shore, and that they are upon opposite sides of the same synclinal axis. There must also be an abundance of limestone in the rocks about the city, because the water from the wells is very hard. Probably the carbonate of lime is distributed in small nodules through the strata as they are in several other localities upon the island.

8. *Granite and Protogine.*—The granite in the south part of Newport is partly granite proper, and partly protogine, only that the talc of common protogine is replaced by chlorite. Some of the felspar of the protogine is greenish in color. Probably the east part of the granite is protogine, and the west part common granite; but we did not examine the distribution of the two carefully enough to separate them upon the map. Much of the granite is very finely grained,—so much so as to resemble red sandstone.

Numerous joints are found in these rocks. Near Newport they run N. and S., and dip 66° E., and very closely resemble planes of stratification. Other directions in the vicinity are the following: N. 20° W., N. 50° E., dipping 29° N. W., and N. 75° E., dip 70° S. In another locality they run N. 70° E., N. 20° E., and N. 45° W., all of which are perpendicular; others dip 65° S. and 65° N. Rarely fragments of slate are found imbedded in the granite, especially near the boundaries of the rock.

There is a coarser variety of the protogine found at the extreme south end of Newport, and extending north as far as Rough Point. This is probably connected directly with the larger mass to the west. At Coggeshall Ledge there are occasionally numerous large crystals of felspar, scattered over the ledges, making the rock porphyritic, and presenting a peculiar checkered appearance. There are also nodular masses or veins of chlorite at the same locality. Large dykes of quartz rock and fine-grained granite are finely exhibited here also, presenting interesting examples of veins slipped from one plane to another repeatedly. At the Dumplings on Conanicut Island, granite appears again in connection with metamorphic slates. The granite in this vicinity seems to have filled exceedingly rugged and irregular fissures.

At the extreme north part of the island in Portsmouth, there is

another locality of granite, of quite different age, almost a gneiss rock. It is older than the coal formation, and of the same age with the granite of the main land of Rhode Island and Massachusetts, very likely Huronian or Laurentian. Yet it is a curious fact that metamorphic slates occur along the edge of this granite.

Origin of the Newport granite. This granite has every appearance of an igneous origin. It is accompanied with the usual evidences of upheaval of the superincumbent strata, and unequivocal proofs of heat, which has altered the rocks adjacent, and the heat seems to have operated with the greatest intensity nearest the granite. Of late, however, most granites are supposed to have been the result of an aqueo-igneous fusion, and we do not think the proofs of absolutely igneous fusion are so marked here as to prevent the belief that the rock was once aqueo-igneous. The presence of chlorite and greenish felspar in the granite, as well as fragments of slate, renders it probable that much of its constituent portions were derived from the melting down of the coal-measures, both the slates and the conglomerate. A year ago, at Springfield, we attempted to show that between sandstone and shale on the one hand, and granite on the other, there is an unbroken series, and that granite is often merely a mechanical rock metamorphosed, and so to speak, melted. We see not why the same suggestion may not apply to the Newport granite.

The period of the metamorphism of the granite and silicious slate is subsequent to the Carboniferous age, for the coal-measures must have been deposited before they could have been altered. Probably the era of the distortion of the pebbles in the conglomerates was the same; and the period itself corresponded with the time when the greater part of the rocks elsewhere in New England were altered, namely, at the close of the Palæozoic age.

9. *Alluvium.* — Much of the alluvium of Aquidneck Island is in the form of unmodified drift. Large boulders are very common. The direction of the drift current did not vary greatly from a due north and south course. The following instances were noted: At the south-east corner of Almy Pond in Newport, the direction of the striæ is N. 10° E.; on Coggleshall ledge, N. 8° E.; at Purgatory, N. 15° W.; near Fort Adams, N. 20° E. Among the boulders specimens of a porphyritic magnetic iron ore, are very common. Their source is at Iron Mine Hill in Cumberland.

The beautiful beaches for which Newport is so justly famous, are deposits of modified drift. Nine tenths of the island is covered with the same geological deposit, insomuch that it is difficult to ascertain the nature of the underlying rocks, except in ravines and along rocky shores. The amount of sand and gravel is also unusually great in the north part of Portsmouth, covering up the junction between the coal-measures and granite. On Newport Neck small beds of rather an inferior quality of blue and white clay may be found by digging into the ground a few feet.

Thickness of all the strata on the Island.—The lowest slates and schists by calculation are 1,000 feet thick. The conglomerate at Purgatory is estimated at 500 feet. The slates and schists overlying it, by measurement with a yard-stick, amount to 473 feet and one inch in thickness. The second conglomerate is 464 feet and one inch in thickness. The next, 920 feet by calculation, are mostly concealed along the line of the section of measurement. Of the coal-measures above these, 1,091 feet and two inches were measured, and 1,577 feet more calculated by trigonometry. The third conglomerate is estimated at fifty feet, and the granite at one hundred feet. The silicious slate by calculation is 1,321 feet thick. This gives a thickness to the carboniferous system of 6,497 feet and four inches; and to the unaltered coal-measures a thickness of 3,588 feet and two inches. Estimating the alluvium as fifty feet thick, the total thickness of all the strata upon the island is 7,547 feet.

We annex a tabular view of all the observations taken of the position of the strata in different parts of the island. The bearings are not corrected for the variation of the needle.

Table of the Dip and Strike of the Rocks upon the Island.

Lower Slates and Grits.

Locality.	Strike.	Dip.
Sachuest Point, east side,	N. 28° E.	45° W.

First Conglomerate.

North of Sachuest Point on east shore,	N. 30° E.	45° — 50° E.
" " " " "	N. 30° E.	dip west.
North of the last locality, in the water,	N. 30° E.	dip east.
Taggart's Ferry (slates),	N. 30° E.	45° — 50° W.
South of Taggart's Ferry,	N. 30° E.	75° W.
Town line of Middletown and Portsmouth,	N. 30° E.	30° E.
Hanging Rocks,		0° — 5° W., etc.
East of Purgatory, in the water,	N. 30° E.	30° E.
Near Purgatory,	N. 30° E.	35° W.
As far southwest as the conglomerate extends, a series of folds,	N. 30° E.	Both E. & W., the west sides of the folds being the steepest.

Slates between the two ranges of Conglomerate.

On the line of the shore from the beginning of the slate to the beginning of the second range of the conglomerate,	N. 30° E.	0° — 55° E.
	N. 20° E.	10° E.
	N. 20° E.	45° W.
	N. 20° E.	10° W.
	N. 20° E.	30° W.
	N. 20° E.	34° W.
	N. 20° E.	31° W.
	N. 20° E.	24° W.
	N. 20° E.	38° W.
	N. 20° E.	30° W.
	N. 20° E.	62° W.

Second range of Conglomerate.

On the west shore of the Point east of Easton's Beach — commencing where the slates cease; ending at the east end of the Beach,	N. 20° E.	62° W.
	N. 25° E.	70° W.
	N. 25° E.	41° W.
	N. 20° E.	48° W.
	N. 20° E.	30° W.
	N. 20° E.	29° E.
	N. 20° E.	26° E.
	N. 30° E.	5° E.

West range of Conglomerate.

Locality.	Strike.	Dip.
East side of Almy Pond,	N. 20° E.	69° W.
North of Alms-house on Coaster's Harbor Island,	N. 30° E.	25° W.
Southwest of Alms-house,		25° southerly.
Northwest of Alms-house,		Horizontal, etc.
North end of the Island,		8° southerly.
Northeast part of the Island,	N. 70° E.	65° N. W.
Miantonomah Hill,		Nearly horizontal.

Coal-Measures.

Black Point, Portsmouth,	N. 30° E.	29° W.
Case's Mine, Portsmouth (Jackson),	N. N. E., S. S. W.	15° — 20° W. N. W.
Aquidneck Mine, Portsmouth,	East of North.	28° — 35° S. E.
Butts Hill, Portsmouth,		A synclinal axis.
Old Mine, Portsmouth,	N. 80° E.	35° southerly.
Near Bristol Ferry,		N. W.
South part of Portsmouth,	N. E. and S. W.	20° N. W.
Slate Hill, Middletown,	N. E. and S. W.	20° S. E.
Lawton's valley, Middletown,	N. 30° E.	15° N. E.
N. W. corner of Gleason's Pond, Middletown,	N. 20° E.	45° W.
North of Gleason's Pond, Middletown,	N. 20° E.	45° W.
Miantonomah Hill,		15° S. E.
Malbone Garden, Newport,	E. and W.	16° N.
Fort Greene, "	N. E. and S. W.	25° N. W.
From the west end of Easton's Beach,	N. 10° E.	30° W.
southwesterly along the shore, as far as the	N. 10° E.	40° W.
metamorphic slates,	N. and S.	48° W.
	N. 10° E.	30° W.
	N. 10° E.	37° W.
	N. 10° E.	25° W.
	N. 10° E.	62° E.
	N. 10° E.	40° W.
	N. 10° E.	16° W.
	N. 20° W.	20° W.
		24° W.
At Barreda's,	N. 20° E.	45° W.
	N. 20° E.	50° W.
Beds of coal,	N. 30° E.	N. W.
	N. 42° E.	
Near the granite,	E. and W.	Southerly.
Pope street, Newport,	N. 20° E.	28° W.
Levin street, "	N. 30° E.	25° W.
Bishop Rock,		45° N. W.
South end of Coaster's Harbor Island,		30° N. N. E.
West side of the Island,		Variable, but nearly horizontal.
Gull Rock,		Northerly.
Rose Island, south end,	N. 20° E.	20° E.
Rose Island, west part (usual position),	N. 50° E.	20° N. E.

Locality.	Strike.	Dip.
Fort Adams (in the fortress),		10° E.
Southwest from the Fort,	N. 42° E.	15° S. E.
" " " "	N. 42° E.	20° S. E.
Castle Hill,	N. 42° E.	28° S. E.
Near S. Bateman's,	N. 40° E.	22° E.
Southwest part of Newport,	N. 20° W.	20° E.
Limestone near Fort Adams,	N. 50° E.	40 — 50° S. E.

Metamorphic Slates.

Near the Dumplings, Conanicut Island,		35° — 40° S. E.
Price's Neck, Newport,	N. 50° E.	41° N. W.
" " "	N. 50° E.	20° N. W.
Near Fort Denham,	N. E. and S. W.	52° N. W.
Near Barreda's estate,	N. 50° E.	N. W.

Jointed Structures in the Rocks.

In silicious slate, Price's Neck,	N. 20° E.	62° E.
" " " "	N. 46° E.	54° N. W.
" " " "	N. 10° W.	90°
" " " "	E. and W.	90°
Cleavage at Price's Neck,	N. 50° E.	30° N. E.
Joints at Price's Neck,	N. W. and S. E.	90°
Near Price's Neck,	N. E. and S. W.	65° N. W.
Cleavage at Fort Greene,		About 40° E.
Joints on the largest island of dolomite in Newport Harbor,		49° W.
Joints at Purgatory (the chasm),	E. and W.	90°
Near the chasm,	N. 74° E.	90°
Near the chasm, most common,	N. 74° E.	65° S. Also 65° N.
Cleavage at Black Point, Portsmouth,	N. 30° E.	62° W.

In granite.

Stratification (?) in granite, N. E. part of Portsmouth,		35° N. E.
Joints near Fort Denham, most common,	N. 18° W.	66° E.
" " " "	N. 20° W.	66° E.
" " " "	N. 50° E.	29° N. W.
North of Lily Pond,	E. and W.	70° S.
West of Lily Pond,	N. 75° E., N. 20° E. and N. W. & S. E.	90° Others dip 65° S. and 65° N.

2. Synchronism of Coal-Beds in the New England and Western United States Coal-Basins. By C. H. Hitchcock, of Amherst, Mass.

A few years ago no one thought it possible to identify any particular bed or series of beds of coal in the Carboniferous system by means of peculiar or characteristic fossils. But now, thanks to several observers and collectors, chief of whom is Leo Lesquereux, of Columbus, Ohio, most of the beds of coal have been found to be distinguished from one another by the peculiar forms of vegetation associated with them. Each series of beds has associated with it either characteristic species of plants, or, more usually, different species, common to several series of beds, but grouped together in a peculiar way.

In the Appalachian and the western coal-fields, the synchronism of the different beds has been largely ascertained, and the equivalency is satisfactory. We refer to Mr. Lesquereux's writings in various scientific periodicals and State geological reports for a statement of the number, order, and names of the different beds in these basins. The equivalency of the New England beds of coal, with the others, has never till now been ascertained. We have made collections of plants from several localities in the New England basin, and Mr. Lesquereux finds that their distribution corresponds to that of the beds in the other basins. The localities examined are in Wrentham (near North Attleborough), Mass., Valley Falls, Portsmouth, and Newport, R. I. The specimens from Wrentham were presented by H. Rice, Esq., of North Attleborough, to the State Agricultural Museum at Boston; where they may be seen as labelled by Lesquereux.

From Wrentham the following species were obtained: — *Asterophyllites lanceolata*, Lesqx.; *A. equisetiformis*, Brgt.; *Annularia longifolia*, Brgt.; *Sphenophyllum Schlotheimii*, Brgt.; *Calamites Suckowii*, Brgt.; *C. Cistii*, Brgt.; *Neuropteris flexuosa*, Brgt.; *N. hirsuta*, Lesqx.; *N. Loschii*, Brgt.; *Alethopteris Pennsylvanica*, Lesqx.; *A. nervosa*, Gopp.; *Pecopteris Miltoni*, Brgt.; *P. arborescens*, Brgt.; *Sphenopteris abbreviata*, Lesqx.; *Lepidophyllum*, nov. sp., and *Trigonocarpon*, nov. sp. Lesquereux says of the locality of these plants: "The exact geological horizon of the shales where these species of fossil plants

have been collected is obvious, not only from the species themselves, but also from their relation in number to each other. It corresponds with the shales covering over No. 3 coal of the western sections of the coal-measures, equivalent of coal D (Lower Freeport) of J. P. Lesley's 'Manual of Coal.'

"The exact counterpart of your shales (or exact likeness) is found especially at both the Salem beds of Pottsville, at W. W. Wood, Port Carbon, and many other places of the anthracite basins of Pennsylvania; and in the western coal-measures; in Kentucky along the Tug river (separating Kentucky from Virginia); in Greenup, Lawrence, Breathitt counties, etc. In the western coal-fields of Kentucky and of Illinois this bed is frequently found with the same fossils, and it is one of the best and most reliable for its coal. In the east, this bed is generally separated into two or three different beds by clay partings of various thicknesses, each bed of clay containing the same or nearly the same plants. It often runs to No. 4, from which in the western coal-fields it is separated by a limestone, and to which it is related by its vegetation — the ferns."

These shales in Wrentham lie above a bed of coal which has been worked, at least a hundred feet. The plants from this bed were not examined, but the probability is, as suggested by Lesquereux before he knew its relative position, that this workable bed is the equivalent of No. 1 B., the big or mammoth coal-bed of the east.

The general position of the Valley Falls bed is the same with that just described. We did not obtain a sufficient number of plants from its shales to authorize a certain conclusion from them.

We made quite a careful examination of the rocks upon the island of Rhode Island, the details of which will be published elsewhere in this volume. The following is the order of strata commencing at the base of the Carboniferous system, and proceeding upwards: Coarse conglomerate containing elongated and distorted pebbles, 500 feet; Slates, shales, etc., 473 feet; conglomerate, 464 feet; measures concealed, but supposed to be slates, shales, and conglomerates, 920 feet. Just above these concealed measures were obtained specimens* of *Pecopteris affinis*, Brgt. (never before found in America); *P. arbores-*

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* To Mr. C. E. Hammett, Jr., and Mr. J. H. Clark, of Newport, we are indebted for these and the following specimens.

cens, Brgt.; *P. unita*, Brgt.; *Asterophyllites sublævis*, Lesqx.; *Annularia sphenophylloides*, Brgt.; *Aphlebia*, nov. sp.; *Sphenophyllum emarginatum*, Brgt.; *S. Schlotheimii*, Sternb.; *Sphenopteris*, nov. sp., and *Annularia fertilis*, Sternb.

Passing over about 660 feet thickness of slates, sandstones, and conglomerates, we next come to fifty feet of shales, associated with numerous small beds of anthracite, and containing *Pecopteris nervosa*, Brgt. Twenty-five feet higher in the series are twenty-five feet thickness of Carboniferous shales containing the *Alethopteris Pluckneti*, Brgt.; and *Neuropteris tenuifolia*, Brgt. The most productive bed of plants is two feet thick, and is 192 feet higher yet. It contains *Asterophyllites sublævis*, Lesqx.; *Pecopteris arborescens*, Brgt.; *Annularia fertilis*, Sternb.; *Neuropteris*, nov. sp., allied to *N. Grangeri*, Brgt.; *Lepidodendron*, *Pecopteris unita*, Brgt.; *P. dentata*, Brgt.; *P. cyathæa* (?), Brgt.; *Sphenopteris elegans*, Brgt.; *Annularia sphenophylloides*, Brgt.; *Sphenophyllum Schlotheimii*, Sternb.; *Cyclopteris*, nov. sp.; *Lepidodendron dichotomum*, Sternb.; *Sphenopteris intermedia*, Lesqx.; *Pecopteris arguta*, Brgt.; *P. oreopteridis*, Brgt.; and two indeterminable species of *Pecopteris*. This latter group corresponds to the plants found at the South Salem bed of Pottsville, or the upper part of the No. 3 coal. The Wrentham specimens, in distinction from them, are from the North Salem bed at Pottsville, or the lower part of No. 3 coal.

The next 1,700 feet of coal-measures are mostly concealed by soil. A few seams of coal have been discovered in them which may correspond with coal No. 4, as it overlies the plants of No. 3 coal. A few species of plants, which are not distinctive, have been found at the top of these 1,700 feet of strata, immediately underlying a conglomerate of fifty feet or more in thickness, whose position corresponds stratigraphically with the Mahoning sandstone of the west. Above this conglomerate there are 1,320 feet thickness of coal-measures within the limits of Newport, in which no seams of coal have ever been discovered. This may be due to the fact of the complete alteration of these measures by metamorphic agency into silicious slate, jasper, chert, serpentine, dolomite, and granite. The total thickness of the whole Carboniferous system in Newport is 6,497 feet.

In the north part of Portsmouth are the only beds of coal that are now worked upon the whole basin — at the Mount Hope Mine. In

the vicinity of this mine there are *eleven* different beds of coal. Above the beds worked for coal three beds of anthracite have been found — and there are six below. From the shales at the mine the following species of plants have been obtained: *Annularia fertilis*, Sternb.; *Odontopteris Brardii*, Brgt.; *Neuropteris*, nov. sp., related to *N. Grangeri*, Brgt.; *Pecopteris arborescens*, Brgt.; *Sphenopteris Gravenhorstii*, Brgt., and several others not yet examined by Lesquereux. These are the plants peculiar to the Lower or North Salem bed at Pottsville.

Thus all the beds of coal and shales containing plants which we have examined in the New England coal-basin belong to the lower coals, and probably all lie below the Mahoning sandstone. Beds (perhaps series of small beds) of coal are found, equivalent to the Pomeroy, South Salem, North Salem, and perhaps the mammoth beds in other coal-basins. We think it doubtful whether the beds of the upper coal-measures of other basins are to be found in the New England coal-field, partly from metamorphism, partly from denudation, or perhaps they may never have been deposited.

Mr. Lesquereux compares the New England basin with the others as follows: "From your very interesting section of Aquidneck Island, it appears that near or at the western limits of the coal-fields of North America, the multiplication of conglomerate strata analogous to that which is found in Nova Scotia is already evident. Thus, your coal-field of Massachusetts and Rhode Island looks as if it formed a link of transition between the coal-basin of the great Appalachian and western region, and that of Nova Scotia. Indeed, the difference is easily marked and understood. To the eastward, the sandstones, conglomerates or *shore materials*, predominate; to the westward, on the contrary, the limestone and marine formation becomes more marked. It is the only difference."

After having read the foregoing paper, Mr. Lesquereux made the following remarks, which I take the liberty to add: "From your section it is evident that the stratigraphical distribution confirms the conclusions drawn from palæontological evidence. It shows that eastward of the anthracite coal-basin of Pennsylvania, there is the same progression in the thickness of the coal-measures, as is everywhere apparent from the Mississippi River, where, for example, the average thickness of the conglomeratic measures is no more than twenty-five feet, to the

anthracite coal-basin of Pennsylvania, where, at Pottsville, the same conglomeratic formations attain to 1,400 feet.

"On the Mahoning sandstone you will please remark, that it is generally a series of strata, like the millstone grit or conglomerate, and not an only or single bed of sandstone. It is generally (but not always) conglomerate at the top only, and sometimes underlaid by one or more strata of sandstone, separated by shales. Thus, it may be at Newport more than 200 feet thick. A remarkable coincidence of the stratigraphical distribution of your eastern coal-measures with those of Pennsylvania, appears in the bareness of the measures overlying the Mahoning sandstone. In the coal-basin of Illinois and western Kentucky, the space (average 500 feet) between the Mahoning sandstone and the Anvil Rock sandstone, which overlies coal No. 12, contains from three to five workable beds of coal which insensibly disappear to the eastward. Thus, at Pittsburg, the space from the Mahoning to the coal is 500 feet, entirely barren of coal. Pittsburg coal is the equivalent of our No. 11 coal of the west. It gradually thins westward, but preserves mostly a good workable thickness. As it is always accompanied by limestone, it is evident that it has been formed under marine influence. It ought consequently to thin out and disappear in the east, where the marine formations are replaced by shore deposits, that is, by sandstones, conglomerates, etc. The coal-measures overlying the Pittsburg coal rarely if ever contain a workable bed of coal. The Waynesburg coal and the Swickley coal are locally three or four feet thick; but it is a local and peculiar enlargement of seams that generally are no more than one or two feet thick. Thus, it is quite a matter of course that you do not find any beds of coal above the Mahoning sandstone.

"Please to examine and compare the general section of the Appalachian coal, in its deepest part, east of Pittsburg, by H. D. Rogers, and the section of the Kentucky basin in Union county, west Kentucky, by D. D. Owen. The base of Rogers's section, the Mercer's coal, is the only part which is not certain, and must be referred perhaps to coal No. 1 A and 1 B. My Second Report on the Geology of Kentucky has fifteen comparative sections of the coal-measures taken at far different places, from the Mississippi River to Carbondale, Penn., and all show the same analogy or rather identity in the distribution of the coal strata; the difference being only in the thickness of the sandstone

strata separating the coal-beds. Of course such a comparison can be made only with the help of palæontology; while before placing two sections opposite each other it becomes necessary first to determine, by identity of fossils, one bed of coal at least, to be equivalent to another of another section. As our coal No. 1 B, has a marked flora, I have generally taken this bed as a point of comparison for my sections, determining it first from the fossil plants. But in some of my sections I have had opportunity for comparing the palæontology of all the strata.

"As I said above, the difference in the sections appears only in the difference of thickness of the strata separating the coal, and also of the coal-beds. But even this difference is not accidental or abnormal. It progresses by uniform increase towards the east and south, and then these differences in the thickness of the coal-measures can be followed, and consequently accounted for. In the north-eastern part of Kentucky, the coal-measures have their median thickness; but by following to the south the Louisa and the Tug rivers, the measures progressively increase in such a way that in Jackson county the distance between coal No. 2 and No. 3 is 165 feet (the average is 100). In Lawrence county, 190 feet; and at Warfield, near the burning spring, southeast corner of Lawrence county, the space is already 230 feet. The space between the other coal strata is just proportioned to this. It is, then, very easy if one cannot follow the development of the coal-measures in every direction and over the whole area which they cover, to be misled at far removed points when we try to come to a comparison of the synchronism of the coal strata.

"It may be that at Rhode Island you have the lower part of the Salem coal, with its upper part separated by 192 feet of measures. As I wrote you before, I could never at Pottsville, Tamaqua, or elsewhere, ascertain exactly the distance or space between both members of No. 3 coal. But I do not find characteristic species enough in the shales just above the 920 feet of measures to authorize the assertion that they belong to No. 1 B or some member of No. 1. They may go with No. 2 coal, and certainly you will be able, by collecting specimens, to find a number of other species. In any case you have a bed of coal, of which the position is perfectly ascertained; namely, that corresponding with No. 3, the Wrentham coal and the coal of Newport 660 feet higher than the top of the 920 feet of concealed measures."

3. Remarks upon certain Points in Ichnology. By Professor Edward Hitchcock, of Amherst, Mass.

Since the publication of Sir Richard Owen's able work on Palæontology, Ichnology may be considered as fairly installed as a distinct branch of this science. It is just sixty years since the first fossil footmark that has been preserved in our cabinets, was disinterred in the Connecticut valley, and only thirty-two years since the first scientific description of such fossils was given by Dr. Duncan. Yet they have already been discovered in nearly all the great rock formations. In the Cambrian rocks we have the trails of Annelids. In the lower Silurian, the tracks of Crustaceans; in the upper Silurian, those of Annelids, Mollusks, Lizards, and Fishes; in the Devonian, those of Lizards, Batrachians, Chelonians, and Mollusks; in the Carboniferous, those of Lizards, Batrachians, and Fishes; in the Permian, those of Lizards, Batrachians, and Chelonians; in the Trias, those of Crustaceans, Batrachians, Lizards, and perhaps Marsupials; in the Oolite, those of Marsupialoids, Birds, Lizards, Chelonians, Batrachians, Fishes, Crustaceans, Insects, and Annelids; in Alluvium, of course, those of all living animals; but those of Mammalia, Birds, Batrachians, Chelonians, and Annelids at least, will be found in the cabinets. These facts indicate great progress; but we predict that in a few years it will seem only the A B C of the subject.

In a branch so recent, it is to be expected that a diversity of views will exist, even as to points of great importance. I propose to make some remarks upon a few such points, on which perhaps my views may differ from those expressed by others.

The first inquiry I shall notice is raised by the enumeration just made of the foot-marks in the different formations. In many cases the character of the animals is not so well settled that cautious writers will admit them into the same catalogue with those determined from their skeleton. Yet no one conversant with the subject, will longer doubt that the tracks were made by living animals, generally of different species from those otherwise discovered. I would suggest, therefore, that those made known by their tracks, be put into a separate group in an Index Palæontologicus. I would call the group

Lithichnozoa, — literally, *stone-track animals*. Geologists would then know the exact amount of evidence on which their existence and characters rest. This course I have adopted in a recent edition of my Elementary Geology.

Some, indeed, take the ground that from tracks alone, we cannot infer the nature of the animal with any such probability as to make it proper to name and describe them, or to put them upon an Index Palæontologicus. We must wait till the skeletons of the animals that made the tracks are discovered.

Let us see whether this opinion has a philosophical basis, and corresponds to the practice of palæontologists.

In the first place, it is certain that we are able to infer with a good degree of certainty the nature of living animals from their tracks. Who would confound the track of man with that of any other species? or doubt that the track of a horse was made by a ruminant with solid hoof, or that of the sheep or the swine by animals with cloven hoofs? or that a tritid thick-toed track, with the normal number of phalangeal impressions and alternating feet, was that of a bird like the three-toed ostrich? or that a similar track, with narrow toes destitute of phalangeal marks and with long stride, was made by a narrow-toed bird? or that five-toed tracks, arranged in two rows and by twos, were made by some mammifer, like the carnivora? or that small tracks in six rows, with linear toes, were made by insects? or that a mere trail was made by a mollusk or an annelid? There could be little doubt in such cases, and we might, as Cuvier said, "regard the conclusion as equally certain with any other in physics or morals." Still stronger, as we shall endeavor to show under our next head, is the evidence sometimes presented by fossil footmarks to show their origin.

2. Suppose we have before us the entire foot of an animal petrified. It might show not only the exact form, but the phalanges, the claws, the heel, and even the striæ and papillæ of the skin. Would not every palæontologist feel as if such a relic of so characteristic an organ, justified him in inferring the nature of the animal, and of giving it a name if new? Indeed, how many of the fossil animals have been described from relics less satisfactory than a well-petrified foot! Yet in many of the tracks we have an exact counterpart of a petrified foot. The deep imprint of the animal's foot in the mud is filled by other mud, which copies the exact form, the phalanges, the claws, the heel,

and even the skin, every part of the foot, indeed, except its upper side; and when the rock is split open, we have this perfect cast, an equivalent, so far as external characters are concerned, of a petrified foot. What philosophical naturalist, then, will say that tracks furnish no reliable data for inferring the nature of the animal?

3. But suppose we find the bones of numerous animals in the same formation as the tracks, how is it to be proved that those animals made the tracks? In no other way but by comparing the bones of the feet with the tracks. But this can be done more successfully, as we think, by making the comparison with the much greater number of the feet of living animals, covered with flesh and skin, as those of the fossil animals were, when they made the tracks. And as a matter of fact, no tracks have ever been identified by fossil skeletons. The only attempt of the kind, that of the Labyrinthodon with the Cheirotherium, appears to us entirely unsatisfactory, highly as we respect the opinion of Prof. Owen, who suggested this identification. For the strait single row in which the tracks of the Cheirotherium are arranged, as given by Owen, never could have been made by the wide frog-like body and sprawling legs of the Labyrinthodon. The fact is, there are insuperable difficulties in this matter, even if we can find fossil skeletons. A few have been discovered in the Connecticut River sandstone; but none of the tracks can be identified with them.

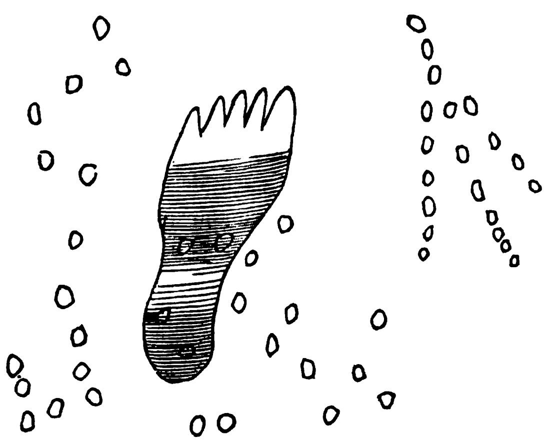
4. It is not strange, in view of such reasoning, that Cuvier should have said, before fossil footprints were known, that "a single foot-mark of a cloven hoof indicates to the observer the forms of the teeth, of all the big bones, thighs, shoulders, and of the trunk of the body, of the animal which left the mark." Nor is it strange that such men as Sir Richard Owen, Sir William Jardine, Professor Kaup, and Isaac Lea, should have named and described animals with no other relic of them but their tracks.

A second point I would notice, is the supposed obliteration of the tracks of the fore feet of quadrupeds by the hind feet brought into exactly the same place. That living quadrupeds do thus bring up the hind foot, I know. It is done most perfectly in the winter, when the snow is covered by a slight crust. But in no case have I seen more than half a dozen tracks in succession, where the hind foot took the place of the fore one so perfectly that marks of both could not be perceived; and rarely is it done in a single case so completely as to

deceive a practised eye. Hence, in reasoning about fossil footmarks, I should say that where we have but few and imperfect specimens, we may mistake a biped for a quadruped from this cause. But if we have numerous specimens, and these very distinct, such mistake is not very likely to occur to one familiar with tracks. A more frequent source of error lies in the fact that the fore feet, in unequal footed animals, does not make so deep an impression as the hind feet; and if we happen to have a specimen with even a thin layer peeled off, the marks of the fore feet may have disappeared.

But I have learned another fact, both in relation to recent and fossil foot-marks, which bears strongly on the point under consideration. Even where an animal makes a deep and distinct impression with his hind foot, in the place just vacated by the fore foot, *the latter is not generally obliterated*, but only sunk deeper into the mud. I will illustrate this by two examples.

FIG. 1.



The drawing here introduced (Fig. 1), shows a man's foot with that of a bird, upon unconsolidated clay from the banks of Connecticut River. On the same surface are rain drops; and though the human foot made a rather deep impression, so as to show distinctly the striæ of the skin, the rain-drops were not obliterated, but only carried bodily deeper into the clay, as the drawing shows.

My second example is derived from the only specimen ever found

of the tracks of the fore feet of the Otozoum Moodii. One of these tracks (that on the right deeply shaded) lies almost entirely beneath that of the hind foot, as this drawing shows (Fig. 2). Yet it is not obliterated; indeed it is quite as distinct as the corresponding track which is free. Prof. Owen must have misunderstood my description when he says, "it would seem that the hind foot, which was larger than the fore foot, obliterated the print of that foot, by being placed upon it in walking" (Palæontology, p. 165). In the case of the Otozoum, the obliteration of the track of the fore foot was not the cause of its infrequent occurrence, but it was rather because the animal rarely brought it to the ground.

FIG. 2.

Such facts conduct to the conclusion that it is unsafe to account for the supposed absence of the tracks of the fore feet by their obliteration by the hind feet. It may occur in some cases, but not as a general fact. Other causes, at some of which I have hinted, account for their absence much better.

A third point on which I would remark, is the position some have lately taken, that none of the tracks in the Connecticut valley were made by birds. It may be so; and no one familiar with the difficulties of Ichnology, should by any means be very dogmatic in, or tenacious of, his opinions about foot-marks. Yet no wise man will hastily abandon an opinion which has been sustained by such strong arguments.

I think but little, indeed, of the popular argument, namely, that some of these tracks look like those of birds. And yet where resemblances are so strong as to arrest every one's attention the moment he looks at them, they should not be set aside hastily as of no consequence. "This is the track of Noah's raven," said Mr. Moody to his neighbors when he showed them, sixty years ago, the first fossil footmarks ever noticed on the globe. "Here are some turkey tracks made three thousand years ago," said Mr. Draper to his neighbor when he first caught sight of the specimens that lay by the sidewalk in Greenfield, twenty-five years ago, and whose description laid the foundation of the remarkable Ichnology of the Connecticut valley. How often have similar remarks been made by intelligent men the first time

they have passed through an Ichnological gallery! Yet every geologist knows how deceptive such first appearances are, especially when seen by men not accustomed to palæontological investigations; and hence multitudes of intelligent men believe firmly that they are looking upon petrified wood, which the mineralogist knows to be some variety of amphibole, horns of deer and stags and petrified wasp nests, which the zoölogist is sure are ancient corals (Cyathophylla and Columnariæ), and petrified rattlesnakes, which the botanist refers to the stems of tropical plants. Not until such supposed resemblances are made to pass the ordeal of scientific investigation, are they of much consequence. How is it with the supposed bird tracks when brought to such a test?

I confine this inquiry to the fourteen species of Lithichnozoa, which, in my Ichnology of New England, I have described under the name of Pachydactylous or thick-toed Birds, whose tracks show only three toes and a uniform and definite number of phalangeal impressions. The evidence as to the ornithic origin of these is stronger than in respect to any others in the Connecticut valley, or anywhere else. The argument to show these creatures to have been birds, is so well presented by Sir Richard Owen in his Palæontology, that I use his language, which, though confined by him to the largest species (Brontozoum giganteum), is equally applicable to them all: "The impressions," says he, "succeed each other at regular intervals: they are of two kinds, but differ only as a right and left foot, and alternate with each other, the left foot a little to the left, and the right foot a little to the right of the mid line between a series of tracks. Each footprint exhibits three toes, diverging as they extend forward. The distance between the tips of the inside and outside toes of the same foot was twelve inches. Each toe was terminated by a short strong claw projecting from the mid toe a little on the inner side of its axis, from the other two toes a little on the outer side of theirs. The end of the metatarsal bone to which those toes were articulated, rested on a two-lobed cushion, which sloped upwards behind. The inner toe showed distinctly two phalangeal divisions, the middle toe three, and the outer toe four. And since in living birds, the penultimate and ungual phalanges usually leave only a single impression, the inference was just, that the toes of this large foot had been characterized by the same progressively-increasing number of phalanges, from the inner to the outer one, as in birds. And as

in birds also, the toe with the greatest number of joints was not the longest; it measured, e. g. 12½ inches, the middle toe from the same base line measured sixteen inches, the outer toe twelve inches. Some of the impressions of this huge tridactylous footstep were so well preserved, as to demonstrate the papillose and striated character of the integument covering the cushions on the under side of the foot. Such a structure is very similar to that in the ostrich."—*Palæontology*, p. 289.

The point of this argument drawn from the number of phalangeal impressions, which from the first we had used in our reasoning, and which Professor Agassiz long ago proposed as a sort of *experimentum crucis* (Am. Journal of Science, Vol. 3, N. S., p. 79), must be regarded as its strongest part. Yet recent discoveries have brought to light tracks, most evidently quadrupedal, which are tridactyle, and show probably the same number of phalangeal impressions. Thus, the Anomoepous, though it had five toes on its fore foot, had a hind foot, which, separated from its long heel, exceedingly resembles the foot of what has been regarded as that of a thick-toed bird, and probably, though I do not feel quite clear on this point, the toes had the same number of phalanges. (See Plate VIII. of my Ichnology). So the hind foot of the Apatichnus, although it has a narrow fourth lateral toe, exceedingly resembles that of a bird; yet the number of its phalanges has not been determined. Both the hind and fore feet of the Plesiornis show three principal broad toes, almost exactly like those of birds, although specimens brought to light since the publication of my Ichnology, exhibit a small fourth lateral toe.

These facts open an interesting field of inquiry, and by some they are regarded as overthrowing wholly the argument for the ornithic origin of all the tracks founded on the phalangeal impressions. But this seems to me too hasty a conclusion. For in the first place, some living lizards have only three toes on their front feet, and probably the extinct Rhynchosaurus had the same number on both its feet, and was even more closely allied to birds in some other respects, especially the nearly edentulous character of its jaws. Secondly, in existing lizards, both Loricoid and Saurian, the phalanges in the three outer toes, when there are only four toes in the foot, and in the three middle toes when there are five, correspond to those of birds; namely, 3, 4, 5, so that if these animals' feet were so modified, as probably they sometimes were, in sandstone days, that they became tridactyle, the inner and outer toes

Fig. 3.

would be those most likely to disappear, and the other three would correspond to those of birds. Thirdly, the fact so clearly established, both by tracks and other fossils, that a large part of ancient animals belonged to groups in which were blended characters now found in several classes; those found for instance in marsupials and birds, birds and batrachians, or lizards, &c., furnish a more reasonable explanation of anomalies in tracks than to deny the ornithic origin of all of them. On this point, however, I shall enlarge shortly. But fourthly, the fourteen species of tracks under consideration exhibit no anomalies, and correspond exactly to those of living birds, except perhaps in the greater distinctness of the phalangeal impressions. Finally, there is the most decided evidence that these animals were bipeds and not quadrupeds, as we must suppose if they were not birds. I have now seen a thousand, yea, I doubt not several thousands of these tracks, many of them most distinct in every part, and generally in rows, yet never have I seen a trace of a fore foot. If only a few of them had been found, it might be probable that the fore foot would yet be discovered; but now it is extremely improbable. Alike improbable is it that the track of the fore foot has been in all cases obliterated. Look at such a row of tracks as that shown on Fig. 3, on the margin, which was copied from a slab in my ichnological cabinet, eight feet long, the stride being twenty-four inches, and inquire whether such an obliteration is probable? And is it probable that tracks so exactly like those of a long-legged wading bird were made by a quadruped? It seems to me that every zoölogist will say that it is hardly possible. Finally, the presumption is, that birds existed during the period of the Connecticut River sandstone, if we admit, as I have endeavored to show in my Ichnology, that it embraced the Oolitic period. For marsupials existed then, regarded as still higher on the zoölogical scale; and Professor Owen has described a fossil bird of the size of the woodcock, from the Green Sand of Cambridge in England, which some place in the upper part of the Oolitic series. There is, therefore, no longer a presumption against the existence of birds in our sandstone as there was when I first described it as triassic.

The evidence of the ornithic origin of the seventeen

species of what I have denominated narrow-toed birds, is certainly not as strong as in respect to the group that has been described; because the phalangeal impressions are wanting. Indeed, some of the tracks of these narrow-toed bipeds have such a resemblance to the feet of some lizards, that I anticipate the discovery of front feet, and their consequent removal into the quadrupedal if not the lacertian class. But if we are satisfied that there were thick-toed birds in sandstone days, the analogies of living animals would infer the probable existence of the narrow-toed also; and when we find so close a resemblance to the tracks of living narrow-toed birds among the fossil foot-marks as in the Ornithopus, Platypterna and Tridentipes, it seems fair to conclude, until the contrary is proved, that some of them were made by birds.

While, therefore, in some respects the argument for the ornithic origin of our foot-marks has been weakened by subsequent discoveries, in other respects it has been strengthened; and while I would not be dogmatic on such a subject, it seems to me to be quite premature to abandon my opinion; which, however, I am quite ready to do when the analogies of science demand it.

Fourthly, I would make a few remarks upon the evidence furnished by Ichnology, of the existence of animals during sandstone days, of types intermediate between existing classes, or rather embracing characters now found only in two or more classes.

The progress of Palæontology, irrespective of tracks, has multiplied examples of such intermediate animals exceedingly, and shown us that the older the formation the more anomalous probably will be the vertebrate animals, which they contain. The Sauroid fishes recur at once to our recollection, which, according to the phraseology of Agassiz, were the prophetic types of Saurian reptiles. Then there came the Ichthyosaurus and Plesiosaurus, the first of which, to use the language of Dr. Buckland, "united in itself a combination of mechanical contrivances which are now distributed among three distinct classes of the animal kingdom." Still more remarkable was the Pterodactyle, "of all the creatures of the ancient world," says Cuvier, "incontestably the most extraordinary." Then there was the Archegosaurus from the coal-measures, whose "head might be that of a fish as well as that of a lizard, or of a batrachian," according to von Meyer, and which seems to have been "a transitional type between the fish-like batrachia and the lizards and crocodiles," according to Goldfuss. The Labyrinthodon

of the new red sandstone, according to Owen, "had the essential bony characters of the modern batrachia, but combined these with other bony characters of crocodiles, lizards, and ganoid fishes." The skull of the Rynchosaurus "has brought to light modifications of the lacertian structure leading towards chelonia and birds, which were before unknown." — *Owen.*

A great desiccated inland sea in Central Africa has brought to light in the Dicynodon, "a race of reptilian animals presenting in the construction of their skull characters of the crocodile, the tortoise, and the lizard, coupled with the presence of a pair of huge sharp-pointed tusks, growing downwards, one from each side of the upper jaw, like the tusks of the mammalian morse." — *Owen.*

Sir Richard Owen has given a general view of this subject in relation to reptiles, when he says, that "a more generalized vertebrate structure is illustrated in the extinct reptiles by the affinities to ganoid fishes, shown by the Ganocephala, Labyrinthodontia, and Ichthyopterigia; by the affinities of the Pterosauria to birds, and by the approximation of the Dinosauria to mammals. It is manifested by the combination of modern crocodilian, chelonian, and lacertian characters in the Cryptodontia and Dicynodontia, and by the combined crocodilian and lacertian characters in the Thecodontia and Sauropterygia."

Now, with so many examples before us, we ought to expect to find evidence of the like intermediate structures in the Lithichnozoa. They have, indeed, struck me from the commencement of my ichnological researches, and the application of this principle to the anomalous tracks, has resulted in far more satisfactory conclusions as to their origin than could otherwise be reached. Thus, when we look at the hind foot of Anomoepus, we are struck with its resemblance to the foot of a bird. But when we see its five-toed front feet, its long heel, and its blunt, stout tail, we are sure it must have been a marsupial, or a lizard, with an ornithoid type. So in the tridactylous (possibly tetradactylous) Plesiornis, the shape of the foot, and perhaps even the number of phalanges, lead us at first to say that the animal was a bird; but when we find that it had four feet, and pellets instead of claws on at least one species, we conclude it to have been a batrachian of an ornithoid type. An inspection of the feet of Anisopus, having the same number of toes and phalanges, and in one species a part of the claws wanting, leads us to regard the animals as closely allied to the crocodilia. But when we

see how unequal are its feet, how nearly on a right line the animals walked, and with what long steps, we are led rather to the marsupials, and hence to call them Loricoid Marsupials. The foot of the Gigantitherium looks much like that of a thick-toed bird, though the curved character of the small hind toe might suggest an analogy with lizards. But the tail trace drives us either to the lizards or marsupials; and the fact that the animal moved nearly on a right line, excites a strong presumption that it was a marsupial, since no living lizard walks thus. No fore foot has yet been brought to light. So like that of a bird appears the principal track of Apatichnus, that the occasional presence of a tetradactylous small fore foot was overlooked; also a tail trace. But all the facts at length forced us to the conclusion that the animal was an ornithoid lizard.

I might add to these examples; but those adduced are sufficient to show how important in ichnology is this principle of former intermediate types of animals. From so narrow a source of information as tracks alone, we may not in all cases be able to say which class of characters predominated in the animals, and therefore may fail to refer them to the right class. But the principle does at least explain satisfactorily certain anomalous characters in the foot-marks otherwise inexplicable, and shows us that ichnology leads to the same conclusions as palæontology in general.

The fifth and last point on which I would remark, is the probable marsupial character of a large number of the Lithichnozoa.

In my Ichnology, I have ventured to describe five species as marsupialoid, without saying certainly that they were marsupials. But subsequent reflection increases my conviction, not only that these species were marsupials, but that I might with strong probability have referred many other species to the same class. I will briefly state my reasons for this opinion; premising, however, that one of the species which I have placed among the marsupialoids, I am inclined to regard as still higher on the mammiferous scale, perhaps even as high as the carnivora. I refer to the Cunichnoides marsupialoideus, which I placed among the marsupialoids, chiefly from the supposed though somewhat doubtful inequality of the front and hind feet. But in every other respect they correspond more nearly with such an animal as the dog.

My reasons for presuming that a large number of the Lithichnozoa will turn out to be marsupials, are the following:

FIG. 4.

The marked inequality of their front and hind feet affords a slight ground for this presumption, because such is the character of many living marsupials. Out of fifty-five species of quadrupeds described in my Ichnology, from their tracks, twenty-five at least had unequal feet. This was the case, also, with many of the lizards and the batrachians, and therefore we need other analogies to give much strength to the presumption that they were marsupials.

The narrowness of the trackway affords a still stronger presumption that those Lithichnozoa, which show this character, were mammals, if not marsupials. Take an example in Fig. 4, on the margin, which is a faithful representation of the track of Anisopus gracilis in the ichnological cabinet. The slab is four feet long and the stride ten inches. It seems to me quite certain that no living lizard or batrachian could walk so nearly on a right line as these tracks are placed. But mammals with long legs, such as the cat and dog, do thus walk. It requires long legs; and such as living lizards and batrachians possess must make two rows of tracks wide apart. A few lizards, such as the Phylliurus cuvieri, might bring their feet more nearly into a line than is usual, because their legs are longer; still they would not make any such rows as the Anisopus and many other fossil species have left. Taking these facts in connection with the unequal feet of many of the species, is not their marsupial origin made probable?

I had thought that even where the above two characters were present in the footmarks, yet we must refer the animals to lizards or batrachians, if there was also the trace of a tail. But I am informed by Rev. George Cook, late president of the college in Eastern Tennessee, who has had opportunities to learn the habits of the opossum, that this animal almost always leaves the trace of its naked tail upon the ground, and that this is the mark by which the hunters distinguish the tracks of this from those of other animals. Hence I should regard as probably marsupial those Lithichnozoa that have unequal feet and leave a narrow trackway, even though they have left a tail-trace.

The recent discovery of numerous genera and species of marsupials in the oölite of Europe, and of some even further

down in the rocks, in Germany and North Carolina, gives increased plausibility to these suggestions as to the Lithichnozoa. For it is thus made probable that the marsupials were numerous in the period when the Lithichnozoa flourished. True the former were quite small, so far as yet discovered. But they may have had contemporaries as large as the Gigantitherium and Otozoum.

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4. On certain Conglomerated and Brecciated Trachytic Dykes in the Lower Silurian Rocks of Shelburne, in Vermont; with special Reference to the Degree of Heat at the Time of their Production. By Professor Edward Hitchcock.

Chittenden county in Vermont, lying along Lake Champlain, contains an unusual number of dykes of trap, and some which cannot be distinguished from Trachyte. Shelburne, a few miles south of Burlington, abounds in them. But I wish to call attention to only one of them of the trachytic type, because it seems to me that it affords us the means of determining the maximum temperature that existed in it when formed. This dyke is at Nash's Point; it is nearly thirteen feet wide in some parts, and at its east end the surrounding rocks have disappeared, and it stands up as a wall along the beach, considerably worn away on the sides, and breached in one or two places. The north side or wall of the dyke, two feet thick, is composed of a dull white trachytic rock, evidently partially decomposed. The south wall is of the same material, about a foot thick. The intermediate space between these walls, several feet thick, is occupied by a conglomerate or breccia, a part of the fragments being quite angular, and a part decidedly rounded or water worn. The fragments vary in size from that of a pea to those four or five inches long, and consist of granite, gneiss, hornblende schist, with garnets, quartz, gray (Potsdam?) sandstone, red rock (Oneida conglomerate?), and Trenton or black limestone. The cement consists of the same materials comminuted and mixed with the trachyte.

We are compelled to call this rock trachyte, because it has the lithological appearance and the chemical composition of that rock. We are

indebted to our friend, Mr. George F. Barker, late assistant in the Yale Laboratory, for the following analysis of it, to which we have added for comparison an analysis of the Drachenfels trachyte, and of trachytic conglomerate from Ofenkuhlen:

	From Shelburne.	From the Drachenfels.	Trachytic conglomerate.
Silica,	67.30	67.09	66.39
Alumina and iron,	19.10	20.22	22.71
Titanic acid,		0.38	
Lime,	0.79	2.25	0.53
Magnesia,	trace	0.97	0.47
Potassa,	4.74	3.56	3.05
Soda,	6.04	5.07	1.94
Loss by ignition,	1.70	0.45	4.89
	99.69	99.99	99.98

The specimen analyzed from the Shelburne dyke was from the wall and not the conglomerate. It is somewhat porphyritic, and the imbedded crystals may be ryacolite, though more probably orthoclase. It must be trachyte we think, though as old probably as the Devonian period.

It is difficult to resist the conclusion that this dyke was at first entirely occupied by conglomerate, and that the trachyte of the walls was produced by the melting down of the fragments; while in the middle the heat was not great enough to do this, and therefore the conglomerate remains. If so, the heat must have been communicated from the rock containing the dyke, and not the reverse, as is generally supposed to have been done.

Fortunately, the nature of the fragments in this conglomerate fixes a limit, beyond which the heat could not have passed. The black limestone fragments still retain apparently all of their carbonic acid. Indeed, carbonate of lime seems to be diffused more or less through the whole rock, since it effervesces somewhat with acids. But had the heat exceeded redness, or 1000° of Fahrenheit, the carbonic acid would have been expelled, as in a lime-kiln. Near the source of heat, that is, the enclosing rock, it may have been higher; though if water with alkalies were present, it need not have been higher to change the conglomerate into felspar.

To point out the facts and conclusions in the last paragraph, was the main object of this paper. But if any inquire, as is natural, how a

dyke could have been filled with fragments, some of which, if not all, have been rounded as pebbles now are by water, we find it very difficult to answer satisfactorily. The fragments were derived from the older Silurian and Azoic rocks around Lake Champlain. They could not obviously have been torn off from such rocks lying above one another in the earth's interior, and thrust upward by the ascending melted matter; for in that case none of them would have been rounded. They must, therefore, have been introduced from above. If a crack were now to open beneath the present Lake Champlain, where detritus has accumulated on its bottom, by the aid of currents, it would ere long become filled with materials not much unlike those in the trachytic dyke. Suppose such a crack did open in early times, beneath an ocean whose bed was strewed with detritus; and suppose the walls of that crack to have had a temperature high enough or to have been subsequently raised to that temperature, aided by the hot water above, partially to melt and metamorphose the contents of the dyke. If any deem this supposition absurd, we can only say that we hope they have one more probable to take its place. We have no other to suggest.

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5. Additional Facts respecting the Clathropteris of East Hampton, Massachusetts. By Professor Edward Hitchcock.

This fossil fern was discovered by my son, Edward Hitchcock, Jr., and described by him in the 22d volume (n. s.), of the American Journal of Science. It occurs on Mount Tom, in East Hampton, where a new locality was discovered by him in 1859. Being about to leave for Europe, he has requested me to state to the Association the new facts respecting this fern, which he supposes the American localities have added to the description given by Adolph Brongniart, in his *Vegetaux Fossiles.*

1. The radiation of the fronds at the top of the fern, as is common in tropical ferns. We have now so many specimens exhibiting this character as to prove it beyond all question.

2. A great and persistent rigidity of the fronds; so that they are almost as often found lying across the laminæ of the rock as in coin-

cidence with them. This fact can be explained only by supposing that the accumulation of sand around the prostrate fronds often did not flatten them down, because so rigid.

3. The stems below the fronds, from half an inch to four or five inches across, are common at the new locality; and though no specimens have been found connected with the fronds, no other plant occurs there, and it is reasonable to suppose the two once united.

4. In some instances these stems proceed from a common base in directions more or less radiated; showing that several of them grew from the same root.

5. The scolloped or perhaps toothed margin of the fronds.

6. Fructification. A few fronds have been found with dots upon them resembling those on the polypodium.

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6. Upon a Diatomaceous Earth from Nottingham, Calvert Co., Maryland. By Christopher Johnston, M. D., Baltimore, Maryland.

P. T. Tyson, Esq., of Baltimore, very kindly sent me for examination a specimen of "Tripoli" from Nottingham, that proves to be an extremely rich diatomaceous earth, containing forms which, if they do not demonstrate its identity with the celebrated Bermuda Tripoli, are sufficient to establish its very close resemblance to the latter, as well as the relationship which many diatomists have recognized as existing between the Bermuda Tripoli and the well-known Richmond Earth. Indeed, I feel assured that the Nottingham is the equivalent, at the least, of the Bermuda Tripoli; and that both, if these be *two*, are derived from the same deposit, which doubtless merges south-westward into the Virginia bed. It may not be amiss to state, in this place, that last summer I visited Bermuda-Hundreds, on the James River near City Point, for the express purpose of finding, if it there existed, the famous Tripoli which bears its name; but my efforts were entirely fruitless.

In the Nottingham Earth the genus Heliopelta is numerously represented; and this genus, together with the species Craspedodiscus elegans, is characteristic of the Bermuda, or, at least, have not been found in any

other deposit as far as I am informed. The Nottingham Earth also contains Aulacodiscus Crux, the importance of which is less considerable than the preceding; and other forms, as Actinoptychus, Goniothecium, Gallionella, Actinocyclus, Pyxidicula, etc., which are by no means peculiar to the Bermuda, but are met with elsewhere, especially in the Richmond earth.

It gives me great pleasure to offer the corroborative opinions of two gentlemen of much distinction as Scientists.

Arthur M. Edwards, Esq., writes me as follows: "Your note of the 14th May, with the enclosed specimens, came duly to hand. The Nottingham Earth is an important discovery, and it corroborates my supposition that the celebrated Bermuda Tripoli came from the same deposit. Your earth is very near the Bermuda."

Through the kind offices of Dr. S. Durkee of Boston, I am in possession of the subjoined highly valuable letter from Charles Stodder, Esq., of the same city, and cabinet keeper of the Boston Natural History Society.

Boston, 20th May, 1860.

"Dear Sir: — I find in the Nottingham Earth from Dr. Johnston the following genera and species: —

Coscinodiscus Oculus Iridis.
" Apiculatus.*
" Lineatus.
" Marginatus ? *
Actinoptychus Velatus.*
" Senarius.*
" Dives.
Actinocyclus, a great variety.
Heliopelta Eulerii.*
" Leeuwenhoekii.*
" Metii.*
" Selligneri.*
Craspedodiscus Elegans.*
" Coscinodiscus.
Eupodiscus Rogersii.*
Orthosira Marina (W. S.).
Septroneis Caduceus.*
Triceratium Reticulatum.*
" Pileolus.
" Acutum.*
" Obtusum.*
" Condecorum.*
" Solenoceros.
Goniothecium Odontella.*
" Rogersii.*
Mastogonia Oculus chameliontis.*
" Actinoptychus.

Pyxidicula Cruciata* and other species.
Pleurosigma.
Systphania Corona.*
" Diadema.*
Navicula Didyma (small).*
Omphalopelta Cellulosa.
Rinoselina.
Periptera, like Bailey's unnamed fig. 15.
Raphoneis.*

Not Diatomes.

Dictyocha Quadratum.*
" Ponticulum.

Polycystina.

Halycaliptra Virginica.
Haliomma ——— ?
Encrytidium ——— ?

Those marked with an asterisk were mentioned by Bailey in his original paper as having been found in "Bermuda Earth."

Mr. Stodder proceeds to say, "If these do not prove the earth to be identical with the Bermuda, it is more like it than any other I know of. A few of the species have not been found in the Bermuda to my knowledge; Actinoptychus dives is a very peculiar species, found by Ehrenberg in Aegina Earth, not in the Bermuda. Craspedodiscus elegans, I believe has never been discovered except in the Bermuda — the same remark applied to the whole of the genus Heliopelta until a few months since, when I found a specimen of H. Leeuwenhoekii in the Virginia Earth. The variety of Coscinodisci and Actinocycli is very great, and will require much study to work them out."

Thus, it is highly probable that we have re-discovered the lost locality, since all evidence declares the striking similarity, if not identity, of the Nottingham earth of Maryland and the Bermuda Tripoli.

I am informed by Mr. Tyson that the bed, to which attention is called in this paper, varies from seven to thirty feet in thickness; and that the extent of the formation, as far as at present known, is about twelve miles from north to south, and seven miles from east to west. Mr. Tyson has not, as yet, been able to determine whether it belong to the upper Eocene or the lower Miocene.

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7. Description of a new Trilobite from the Potsdam Sandstone. By Frank H. Bradley, of New Haven, Conn.; with a Note by E. Billings, of Montreal, Canada.

Conocephalites minutus (n. sp.).

Fig. 1.

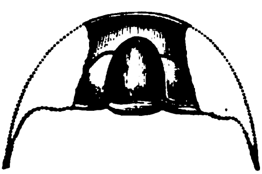

Fig. 2.

Fig. 3.

Fig. 1. The head magnified. The dotted lines represent the supposed outlines of the parts not preserved.

Fig. 2. The pygidium magnified.

Fig. 3. A detached cheek, magnified.

Cephalic shield apparently semi-circular, or nearly so; anterior

margin, as far as preserved, with a narrow slightly elevated rim, just within which there is a rather strong groove. Glabella conical, slightly narrowed at the neck segment, three fourths the whole length of the head, very convex and obtusely carinated along the median line. Neck segment rounded and prominent; neck furrow narrow, but well defined. There are two pairs of deep glabellar furrows which are inclined inwards and backwards at an angle of about 45°; their inner extremities distant from each other rather more than one third the width of the glabella. The anterior lobe is a little less than one half the whole length of the glabella, excluding the neck segment; the two posterior pairs are nearly equal to each other. The glabella is distinctly separated from the cheeks by a narrow, deep groove, which extends all round. From the anterior lobe on each side a narrow filiform ridge curves outwards and backwards on the fixed cheek to the edge of the portion preserved. The eyes appear to be situated just where these ridges terminate as represented in Fig. 1. Judging from the portion of the eye preserved in a detached cheek-plate, its form is semi-annular, and its length at least one fourth that of the glabella. Its distance must be at least one half the width of the glabella. Caudal shield nearly as large as the head, its width scarcely equal to half its length; the lobes nearly equal; the middle lobe very convex with five sharp transverse grooves; the side lobes somewhat flat, and each with five grooves.

The largest head discovered is exactly two lines in length.

The course of the facial suture has not been ascertained. The surface of the glabella in one of the specimens appears to be smooth, but in none of the others can it be distinguished.

The specimens are mostly in a clayey layer, which is full of fragments of all degrees of perfection; in one specimen I count ten heads and three tails, all in a fair state of preservation. In two instances, I have found the casts of maxillary plates, showing distinctly the elevated margin, of one of which I give a figure.

The original specimens were collected (at High Bridge, near Keeseville, N. Y.) in August, 1856, while on a geological excursion with Col. Jewett of Albany, but were not recognized until July, 1857, when a second visit to the locality secured a few *casts* in the solid sandstone, none of the clay layer being obtained. By the kindness of Prof. J. D. Dana, the specimens were presented at the American Association

for Advancement of Science at Montreal, but were not recognized as belonging to any known species. Since that time, I have looked for descriptions, but cannot find any to correspond.

At the same locality, I also procured the cast of a Pleurotomaria, and one of what seems to have been a plate from the stem of a crinoid.,

Note by E. Billings. — Mr. Bradley having favored me with a view of his very interesting specimens, I think there can be no doubt but that they belong to the genus *Conocephalites*. If this reference be correct, then we have at least three, if not four species in North America.

1. *C. antiquatus* (Salter), described from "a cast in a brown sandstone, said to be a bouldered fragment from Georgia." (See Quart. Jour. Geol. Soc. vol. XV. p. 554.)

2. *C. minutus* (Bradley). In this species, the form of the glabella and its proportions in relation to the length of the head are almost precisely the same as in *C. antiquatus*, and yet I think the two are not identical, for the following reasons: In the first place, all the specimens of *C. minutus* are of a nearly uniform size, the length of the head being about two lines, and, therefore, it seems probable that they are the remains of adult individuals. The total length would thus be about half an inch, while Mr. Salter's species is full one inch and three fourths. In the second place, the distance of the eye from the glabella, in *C. antiquatus* is only one third the width of the glabella, but in *C. minutus* it must be at least one half the width. These are the only differences that can be well made out, from the imperfect specimens, but they seem to me sufficient to indicate two species. Mr. Salter says further, that the lobes of the glabella in *C. antiquatus* are very obscure, and that the ocular ridge, if any existed, must have been very slight. His specimen was somewhat abraded. In *C. minutus* the ocular ridge is, for so small a species, very strongly defined, and the glabellar furrows are so deep that it would require a very considerable amount of abrasion to obliterate them.

3. *C. Zenkeri* (n. sp.). This is a new species recently discovered in the magnesian limestone near Quebec. It will probably be described in the next number of the Canadian Naturalist and Geologist.

4. There is in the collection of the geological survey of Canada, a plaster cast of the surface of a fragment of rock which holds four

specimens of a trilobite, each about the size of *C. antiquatus*. They appear to me to belong to the genus *Conocephalites*. The original specimen was collected in Newfoundland, in the same slate that holds *Paradoxides Bennettii* (Salter), and I am informed that it is in the possession of a gentleman who lives somewhere in the United States, but whose name or place of residence, I have not been able to ascertain.

Of the above four species, Mr. Bradley's is at present the most important as it fixes indisputably, at least one point in the geological range of the genus on this side of the Atlantic. In Europe, *Conocephalites* has not been found out of the *primordial zone* of Barrande, but the Quebec and Keeseville specimens show that here it reaches the Lower Silurian.

Additional Note by E. Billings.—Since my note to Mr. Bradley's paper was written, he has collected quite a number of new specimens of *C. minutus* at the same locality. At his request I have examined them and find that they exhibit several of the parts not preserved in those upon which the original description was founded.

FIG. 4 (Nat. size).

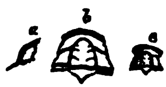

Fig. 4. *a* A detached cheek, showing the small spine of the posterior angle.
b c Two specimens of the glabella, showing the spine on the neck segment.

1. The posterior angles of the head are produced into short spines, as we supposed, but these spines, instead of being elongate-triangular are sub-cylindrical or needle-shaped and projected outwards at an angle of 45° or thereabouts, to the longitudinal axis of the body. The cheek does not appear to be striated but rather smooth. These two characters furnish additional grounds for separating the species from *C. antiquatus* (Salter), which has the cheeks striated and the posterior angles of the head only slightly produced into short broadly triangular terminations.

2. The neck segment bears a short broad-based spine. The first specimens collected do not exhibit this, but on reëxamining them I think I can see traces of it. Some of the specimens of *C. coronatus* (Barrande) lately collected in the Primordial Zone of Spain have a

spine on the neck segment of the same form as that of *C. minutus*, while others (according to the figures) have not; and it may be that individuals of our species will yet be discovered in which the absence of the spine can be clearly established. This remark is made here because on comparing the figures of the Bohemian and Spanish specimens of *C. coronatus* it would appear that the presence or absence of a spine on the neck segment is not always of specific importance and should some of those from Keeseville turn out to have only a plain neck segment, we would not perhaps on that ground alone be authorized to constitute two species.*

3. Mr. Bradley's new specimens also show that there are three pairs of glabellar furrows, the anterior being represented by two small indentations just in advance of the points where the ocular ridges reach the glabella; and further that the course of the facial suture is the same as it is in *C. striatus* (Emerich). The pygidium is more obtusely rounded than is represented in our Fig. 2.

As to the correctness of the generic reference of this species, it may be remarked that Barrande is of opinion that no less than eleven of those which Angelin has figured under the genera *Solenopleura*, *Eryx*, *Conocoryphe*, and *Harpides* should be placed in *Conococephalites*. In this view of Barrande's, Angelin has concurred.† The genus has thus been greatly extended, and judging from the form of the head (and more particularly of the glabella) of Angelin's species *C. brachymetopus*, *C. homelotopus*, and *C. canaliculatus*, I think we are perfectly justified in referring this species to *Conocephalites*. The genus is most closely allied to *Calymene*, having the same number of segments in the thorax — the same number and arrangement of pieces in the head and the same general form and lobation of the glabella, the differences between the genera consisting principally in certain characters of the pleuræ and hypostoma ‡ to which may be added the ocular ridge

* Compare the article, *Sur l'existence de la faune primordiale dans la chaine cantabrique*, par M. Casiano de Prado; suivie de la *Description des fossiles*, par MM. de Verneuil et Barrande. Bulletin Geol. Soc. France, 2e Series, vol. xvii. p. 516 (1860). And also Barrande's *Système Silurien*, plate 13.

† See Barrande's "*Parallèle entre les dépôts Siluriens de Bohême et de Scandinavie*," p. 19; and compare the tables on pp. 17 and 35 of the same work. See also, Angelin's *Palæontologia Scandinavica*.

‡ See Barrande, "*Système Silurien du centre de la Bohême*," pp. 417–419.

which although not a constant character in *Conocephalites*, may be regarded as of some generic value, as it does not occur at all in *Calymene*. I would also state, that since examining Mr. Bradley's recent collection, I have been strongly impressed by the resemblance between the form of the cheek and small needle-shaped posterior spines of *C. minutus* and the same parts of the head of the Quebec species which I have called *Menocephalus globosus*, and it appears to me that *Menocephalus* must be regarded as another closely allied genus. If we except those two genera, *Calymene* and *Menocephalus*, there is no other but *Conocephalites* to which our new trilobite bears any near affinity.

II. PHYSICAL GEOGRAPHY.

1. Remarks on the supposed Open Sea in the Arctic Regions. By W. W. Wheildon, of Charlestown, Mass.

That which has frequently been suggested in the geography of the Arctic regions as probable, seems of late years to be freely admitted as a theory, namely, an open Polar Sea. In fact, such is now the evidence in support of an ameliorated climate around the North Pole, that the existence of an open sea, in or near the centre of the Polar basin, is regarded by many as an established fact in physical geography.*

The theory of Lieut. Maury, to some extent adopted by others, on this subject, appears to be that the waters of the Gulf Stream, are in some way, through Baffin's Bay and the Spitzbergen Sea, carried into the Arctic Ocean and by their influence maintain an open sea at or near the pole. He says, "there is an under-current setting from the Atlantic through Davis's Straits into the Arctic Ocean," and that "Wrangle's Polynia, to the north of Siberia, if it exist, probably owes its freedom from ice to the warm waters of the Gulf Stream, which

* Some, however, still doubt the existence of an open sea. A writer in the Edinburgh Review, 1857, says: "We are bound to say, however, that this *notion* of an open sea still awaits further confirmation." Dr. Rink (Royal Geo. Soc.) doubts the existence of an open sea, "assumed to be kept open by a branch of the Gulf Stream, from Nova Zembla, down Smith's Sound to Baffin's Bay," &c.

run between Spitzbergen and the North Cape into the Arctic Ocean." * Professor Bache seems to give some countenance to the theory here suggested.† Dr. Hayes has repeatedly expressed himself in favor of it; but his great predecessor, Dr. Kane, believing in the evidence which points to the existence of a milder climate and an open sea near the Pole, is not disposed "to express an opinion as to the influence which ocean currents may exert on the temperature of those far northern regions."

Without attempting any controversion of this theory, always so unsatisfactory, we may observe that it does not appear to be adequate for the purpose proposed, even if there were no other objections to it. It seems to us that, while the influence of the Gulf Stream, as such, has been unduly enlarged and overrated, *as regards the Arctic Regions*, the influence of the *Currents of the Air*, beyond a certain parallel of latitude and beyond certain admitted purposes, has been mainly overlooked; and we are unable, from such investigations as we have been permitted to make, to resist the conclusion that an open sea, or an ameliorated climate, at or near the position of the theoretic pole of the earth, is largely if not entirely due *to the currents of the air from the equatorial regions, which move in the higher strata of the earth's atmosphere, bearing heat and moisture with them.* ‡

The circulation of the air from the equatorial regions towards the poles of the earth, and thence to the equator again, for the purpose of modifying the cold of one region and the heat of the other, is no new theory, though it would seem that its proper influence, especially in the northern regions, where by far the most numerous explorations have been made, has never been fully admitted. Dr. Buist, more adequately perhaps than any other writer, admits this influence. Dr. Hayes recently said, in one of his public lectures on the subject, "If it can be shown, as assumed, that a milder climate exists about the Pole, it must

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* Maury, "Currents of the Ocean," and Letter of Instruction to Lieut. De Haven.

† In a recent letter (May, 1860) from Prof. Bache, he expresses his hope that Dr. Hayes may reach that open sea, "where our great Gulf Stream rests from its labors after carrying the warmth of the equator to temper the cold of the pole."

‡ How far the flow of these currents of air from the exact position of the equator, or either north or south of it, may be affected by the physical constitution of the earth, and the disproportion of land between the northern and southern hemispheres, may hereafter be an interesting subject of scientific investigation.

be owing to the influence of open water;" but Dr. Kane, in speaking of the phenomena of the Arctic regions, more justly remarks, "To refer them all to the modification of temperature induced by the proximity of open water is only to change the form of the question; for it leaves the inquiry unsatisfied, What is the cause of the open water?" It is remarkable that the friend and follower of Dr. Kane did not see this difficulty, and the inconclusive nature of his remark.

In offering some of the more prominent suggestions in support of the theory of the currents of air, perhaps the first to be spoken of is the prevalence of the warm element in winds blowing from the north and north-east (true). These winds, with some variations and still more uncertainty of compass-direction, are mentioned so generally by Arctic explorers, and confirmed by whalers and others, that the fact of the extraordinary presence of warm air in those frozen regions, in mid-winter, may be considered as fully established. The result obtained from the recorded observations of Dr. Kane, discussed by Mr. Schott, is, "that the south-east (magnetic), north-north-east (true) winds had the effect of elevating the temperature of the air even in the winter months, which [says Mr. Schott] may be supposed to have arisen from its originating or blowing over a water area, partially open (this water would have a surface temperature of 29° Fah.). The direction points across Washington Land and Kennedy Channel, as the seat of this influencing area."* Sir Edward Parry also found at Melville Island, Winter Harbor, that the northerly winds were often warm. But the experience of Capt. McClintock, in his recent successful voyage of search for the remains of Sir John Franklin's expedition, is still more remarkable, and his own suggestion, the first to be found recorded in any similar work, was welcomed by us as a most satisfactory illustration of a theory so long entertained.† The following is Capt. McClintock's statement: —

"Nov. 1857, Melville Bay. 23d. A heavy gale commenced at N. E. on the 21st, and continued for thirty-six hours unabated in force, but changed in direction to S. S. W. It appears to have been a revolving storm, moving to the N. W. Yesterday, as the wind approached

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* Assistant Charles A. Schott, letter to A. D. Bache, dated Dec. 4, 1859.

† The writer has entertained his theory for many years, and made a public statement of it before Capt. McClintock sailed on his voyage.

S. E. the temperature rose to + 32°; the upper deck sloppy; the lower deck temperature during Divine service was 75°!! As the wind veered round to S. S. W. the wind moderated, and temperature fell: this evening it is —7°. How is it that the S. E. wind has brought us such a very high temperature? Even if it traversed an unfrozen sea it could not have derived from thence a higher temperature than 29°. Has it swept across Greenland — that vast superficies partly enveloped in glacier, partly in snow? *No, it must have been borne in the higher regions of the atmosphere from the far south, in order to mitigate the severity of this northern climate.* [The italics are ours].

"Petersen tells me the same warm S. E. wind suddenly sweeps over Upernavik in mid-winter, bringing with it abundance of rain; and that it always shifts to the S. W.,* and then the temperature rapidly falls; this is precisely the change we have experienced in lat. 75°. I believe a somewhat similar, but less remarkable, change of temperature was noticed in Smith's Sound, lat. 78¾° N." — pp. 63, 64.

The constant occurrence of fogs throughout the Arctic regions, and especially towards the north, at all seasons of the year, is remarked by all explorers. Dr. Hayes says:

"The record most commonly is, a most curious phenomenon was witnessed upon the north horizon: a heavy mist-bank floated there almost continually, and 'mist-bank in the north horizon' became a stereotyped entry upon our meteorological tables."

If we suppose these fogs to be regarded, as they generally are, as arising from open water, they only go to indicate the existence of such open water, which they do not in any degree account for. Some of the lesser fogs are, no doubt, local, produced over small openings in the water, wherever such occur from any local cause, and these are sometimes denominated "ice-smoke." Falls of snow and rain are of frequent occurrence, and in some cases of record are reported almost daily.† Storms of great violence rise suddenly, and in many cases as suddenly cease. It is remarked by some navigators, who have spent

* The points of compass here given are probably magnetic, but these are so variously stated by explorers, in regions of such great variation, that they are often unreliable and not to be depended upon.

† Mr. Schott, in his paper on Dr. Kane's observations, says: "In seventeen months [in one locality, Van Rensselaer Harbor], it snowed 680 hours, and rained during sixty hours. Snow fell in all the months of the year, but rain only in July."

months in the Arctic regions, that they do not remember to have seen the seas and the horizon both free from cloud even in the clearest weather. In the year from September, 1853, to September, 1854, in lat. 78° 40′ about, Dr. Kane records only seventy-three clear days, averaging six to each month, the most of these in the winter months, and the largest number (thirteen) in November. In the succeeding eight months, from September, 1854, to April, 1855, both months inclusive, fifty-four clear days were noticed, seventeen of these in December. Capt. Beechey, June 26, 1818, says: "We had a fall of snow, and at noon, for the first time since crossing the Arctic circle, a shower of rain, which, although the summer was so far advanced, cased every rope in ice as it fell." The rain, as is often the case in lower latitudes, was warmer than the air. After the rain "the gale abated, and the next day it fell calm." It would seem that these constant fogs and the frequent falls of snow and rain, and especially the changes from snow to rain (reported more particularly by Capt. Parry and others), can scarcely be accounted for on any satisfactory basis without admitting the presence of warm air and moisture from the equatorial regions.

There are in the northern regions of America, Europe, and Asia, a number of large rivers, which flow northwardly into the Arctic Ocean. These rivers must necessarily be kept open for a considerable portion of the year, and the ice and snow which accumulate about their sources and along their banks, for their supply, must be melted by some influence, and their waters returned to the ocean — and this in and through regions of almost perpetual congelation. It seems to be almost impossible that the sun, during his continuance above the horizon and from the indirection of his beams, can exert sufficient power to effect this necessary purpose, either among the mountains or in lands whose general slope must be towards the Arctic Ocean. The warm air in the equatorial currents, and the warm rains which they bring, must do here that which the sun by his presence cannot possibly accomplish.

The equatorial regions are considered to include, north and south of the equator, some forty-seven degrees of latitude; and the air which rises into the atmosphere from this vast region is reported to be of different degrees of temperature, varying from 100° to 70° of Fah. and evaporating from the oceanic surface from one hundred to thirty-seven inches of water annually, as an approximate average. Of course, the currents of air will rise to different heights, depending upon its rarefaction and moisture, and be subject to the influence of the different states

of the atmosphere which they may reach. The less rarefied, parting with its caloric and moisture more quickly, will descend in lower latitudes; while that of higher temperature, reaching higher regions, must descend in higher latitudes, at or near the poles of the earth. It will be remembered that this air from the equatorial regions rises, more or less equally, from the vast space indicated by the lines of latitude, and from the entire circumference of the earth at its greatest diameter, converging as it approaches the poles, correspondingly it may be to the lines of longitude, and subject of course to be influenced by the diurnal motion of the earth. The currents from opposite directions, high in the atmosphere, and descending to supply the place of the air moving towards the equator, to maintain the circulation, must necessarily meet and counterbalance each other, producing those calms in the northern regions said to occur more frequently than the winds from all directions added together. Passing from the central regions of the earth, where its diurnal velocity is the greatest, it is subjected to a slower movement as it approaches the poles; and from its convergence and intermingling, with varying proportions of heat and moisture, as from the burning regions of Africa on one side, and the moist surface of the Pacific Ocean on the other, would be likely to produce much of that elemental strife and other remarkable phenomena of the Arctic regions, not otherwise accounted for.

The poles of greatest cold in the north are located on the American and Asiatic continents at about lat. 78° (though probably varying from this degree), and are found to correspond, approximately at least, with the magnetic poles. The position of these poles of cold, so many degrees south of the pole of the earth, not heretofore accounted for, does not seem to conflict with the theory of the currents of warm air from the equatorial regions, but rather to strengthen it. While an open sea, or an ameliorated climate, would be maintained at or near the pole by the influence of the descending air and fall of moisture, of a comparatively high temperature, the region further to the south, *over* which the equatorial currents, when not otherwise influenced by opposing currents, would generally pass, would necessarily be the region of extremest cold — because too distant to be reached by any direct or surface influence from the warm latitudes of the south, and likewise distant from the immediate action of the descending currents of warmer air at the north. The warming influence of the sun cannot have the

effect it exerts in lower latitudes, and the warm air from the equatorial regions passes over and beyond it.*

The Arctic Ocean, it seems, to the north of a certain limit, — varying doubtless in different years, and more or less at different seasons, — is surrounded by an ice-belt, and so far as explored, encumbered with ice in masses of greater or less magnitude. It has heretofore been supposed that a large portion of it, near and around the pole, is covered with solid masses or extensive fields of ice, and Capt. Parry, with all his experience, in 1827, expected to be able to travel over these fields even with reindeer to draw his sledges, to the region of the pole ! He expressed his disappointment in this respect, when, after travelling twenty days over the hummocks and through the floes with incredible labor, to lat. 82° 17′ 10″ (without his reindeer), he "began to doubt whether he should at all meet with the solid fields of unbroken ice which every account had led him to expect at a much lower latitude." Capt. Parry made in this expedition the highest northing which has ever been made, and it appears from his account, as from others, *that* the Arctic Ocean, as far as explored, is covered with hummocks and masses of broken ice, excepting, perhaps, some uncertain statements of open water seen in different directions, and the reported absolute view of the sea by Morton.

Among all the attempts which have been made in a period of more than 300 years, to explore the Arctic regions, the attempt of Capt. Parry, with sledges and boats, in 1827, is one of the most remarkable; and if his "excursion," as he called it, proved any thing, it proved that the currents (about which there prevails the greatest uncertainty, and most conflicting testimony), set *out* of the Arctic Ocean, and that an ameliorated climate was likely to be found at the north, where, from his experience, a conclusion might be drawn that it rained almost continually. On the third of July, after recording rains almost every day, in lat. 82° 3′ 19″, he says the snow had changed to a heavy rain [?], and "had produced even greater effect than the sun in softening the ice."

* This point, as well as some others in this discussion, may be enlarged upon hereafter, when the positions of the poles of cold are more fully illustrated, and the isothermal lines of the Arctic Regions better established, though neither of these will probably be more than approximately determined at any period. It will be an important fact to know hereafter, whether if the poles of cold are found to be changeable, the magnetic poles will be found to be changeable also.

On the next day, they were again annoyed by a heavy rain, the thermometer in the shade at 35° and 36°; and in reference to the frequent rains, at a point further north than ever reached before or since, Capt. Parry remarks, that "he had experienced more rain in the course of this summer than during *the whole of seven previous summers taken together, though passed in latitudes from seven to fifteen degrees lower than this,*" — where probably the rain fall would be less. He evidently did not look for rain so far north as he had reached, but rather for boundless fields of ice, on which he might pass over the very axis of the earth. The ice, however, became so thin and rotten that only one person was allowed to pass from floe to floe at a time, and the baggage and provisions were transported with the greatest labor and caution. He had rain nearly every day, and found large ponds of fresh water on the ice, which appeared to be lower than they had previously been. Water-birds and gulls were seen here, and he mentions as a matter of "ridiculous importance" the finding of two small flies on the ice! Capt. Parry travelled 292 miles in thirty-five days, but owing to his circuitous course and the drift of the current, was only 172 miles from his ship at Spitzbergen; and as he could make no further progress against the current, over the broken and unsafe ice, was compelled to return, aided by a drift of several miles per day.

Thus ended this remarkable "excursion," — a total failure as to the attainment of its object, but apparently proving the existence of an ameliorated climate at the north, and, it may be, demonstrating the possibility of reaching the region of the pole by some other route and better arranged means. As regards its results, they seem to afford no ground to sustain the Gulf Stream theory of the open sea, if they do not wholly unsettle it.

There is not in all physical science, probably, a more sublime arrangement, illustrative of the wisdom of the Creator, for the most beneficent of purposes and the accomplishment of such important ends, than that which we have been endeavoring to illustrate, namely, the circulation of the air from the equatorial regions to the north and south extremes, where the sun's rays do not exert that genial and life-giving influence with which it directly endows other portions of the earth. We have now partially and in a somewhat cursory and unscientific way, presented some views pertaining to this subject and bearing upon the question of an open sea, or ameliorated climate, at the north, — which,

however small their claim to originality, or right to be regarded as a theory, may yet be entitled to some further consideration. There are many facts and groups of facts, in physical science and meteorology, recorded in the numerous works of Arctic explorers and scientific writers, more or less connected with this subject, to which we have not even alluded. These may yet be considered, and their proper weight allowed upon the views presented. Of one thing, however, we may feel quite certain: the purposes of the great heat of the equatorial regions, once produced and carried into the air, not there to be annihilated, cannot be wholly misunderstood; and those purposes, it seems to us, are unaccomplished if their influence be not visible, as we have endeavored to show, at the extreme ends of the earth, towards which they tend and where their influence is most required. Unless this be so the sceptic may declare the wisdom and power of God unequal to his beneficent purposes, and the polar regions may really be what some philosophers have supposed, not only regions of utter darkness, but of perpetual stagnation and death.

II. ZOÖLOGY.

1. The American Reindeer. By L. E. Chittenden, of Burlington, Vermont.

Rangifer caribou, *Baird, Aud. & Bach.*
Cervus tarandus, *Richardson.*
Cervus hastalis, *Agassiz.*

Dr. Richardson, whose opportunities for the study of the fauna of Northern America, have equalled those of any other naturalist, has partially described two varieties of this ruminant, under the names of the "*barren ground*" and "*Woodland*" caribou. Professor Baird adopts, with some hesitation, the same division into varieties, and gives to the former the name of *grœnlandicus*. He adopts the description given by Audubon and Bachman, not having the means of furnishing an original account of either animal.

Although specimens are not very difficult to be procured, the caribou

appears to be less accurately known to naturalists than any other of the American *Cervidæ*. Almost the only description that has fallen under my notice, which has much claim to scientific accuracy, is Dr. Richardson's. That is quite imperfect and erroneous. Some of its errors I hope to correct in the present paper.

I have recently made a thorough examination of two specimens of what is called the "*sylvestris*," or woodland variety, and have seen several skins and antlers, of what were supposed to be specimens of the smaller species. I have also had a very considerable correspondence with reliable gentlemen in various parts of the British American provinces, acquainted with the habits of this animal, whose accounts, written, or given under circumstances precluding consultation, present too striking a coincidence on all points of importance, to admit the suspicion of inaccuracy of description. The information thus derived, has been compared with such published accounts as have been accessible to me, and the results are given in the present paper.

Geographical distribution.

There is no doubt that the caribou inhabiting the so-called *barren grounds* of British America, is much inferior in size to the animal found in the wooded portions of the country. I use the terms *woodland* and *barren ground*, only to indicate this difference in size, without intending to assert that one differs in any other respect from the other. My views on that subject will be hereafter expressed.

The limits of the *habitat* of the American reindeer cannot be well understood, without a glance at the physical peculiarities of the country, to the northward of the forty-eighth parallel of north latitude. A district lying between the coast of Labrador and Hudson's Bay, and another lying between the sixty-second parallel and the Polar Sea, pass under the general name of the *barren grounds*. These are traversed by a few rivers, some of whose banks are unwooded, while others are skirted by a narrow growth of stunted trees. Throughout these regions, numerous conical hills of slight elevation are distributed, formed of the primitive rock, the valleys between which sometimes contain a soil of sufficient strength to sustain a dwarfed growth of the willow and black spruce, but more generally the soil consists of the *debris* of rocks, which is a coarse, dry quartzose sand, unfit for the

support of any thing but lichens. The Hudson's Bay Company, whose posts are maintained throughout the habitable portions of the British provinces in America, with difficulty maintains stations at points widely separated, on these lands, during portions of the year, while a few wretched families of Esquimaux and Chipewyan Indians, termed from their mode of subsistence, *caribou eaters*, are the only human beings that constantly reside on them. A vivid description of the northern portion of this arctic desert, lying between Melville Peninsula on the east, and Mackenzie's River on the west, is found in the account given by Dr. Richardson of his overland journey in search of Sir John Franklin in the year 1848. The characteristic inhabitants of these lands are the *musk ox* and the smaller *caribou*, and in their almost unbroken solitudes, the latter animal is found in numbers approaching those of the bison herds upon the north-western prairies. The rivers which discharge their waters into the northern portion of the Gulf of St. Lawrence, penetrate a section of this district, and on their banks this animal rears its young as far south as the fifty-first parallel of north latitude. Gentlemen whose authority may be confidently trusted, who are accustomed to resort to these rivers during the season of salmon fishing, inform me, that caribou are frequently seen on the plains, at short distances from the river banks, in numbers varying from single pairs to hundreds.

From Fort William, on Lake Superior, there is a route of water communication, broken by numerous *portages* extending to the northwest, through Rainy Lake, Lake of the Woods, and Lake Winnipeg to the Saskatchewan River; thence via Lake Athabasca and the Great Slave Lake to Mackenzie's River, which is very near the eastern limit of the great limestone deposit, which extends far toward the eastern slope of the Rocky Mountains. Between this line and the barren grounds is a wide extent of country, sometimes called the "Eastern district." The well-known deflection of the isothermal line to the northwest gives to this limestone region, as far north as Peace River in latitude fifty-seven north,. a climate having a mean temperature scarcely colder than is found on the meridian of Boston in latitude forty-five degrees north. The alluvial valleys of this region are covered with a heavy arborescent growth, not unlike that of the valleys of New England. To the northeast of this line, however, the country presents a far different physical appearance. Its southern portion,

south of a line in the course of the St. Lawrence and the Great Lakes, and from three to five degrees distant from them, is the great Pine District of the Canadas. To the north of this line, is a wide extent of elevated country, which is drained to the south by the Saguenay, Aux Lievres, Gatineau, Ottawa, and other rivers, and to the north by numerous streams discharging their waters into Hudson's Bay and the Polar Sea. This country is for the most part made up of extensive morasses, occasionally broken by slightly elevated ridges. In these morasses are many *islands* of comparatively solid ground, to which have been given the expressive name of "marsh cakes." These are covered with a dwarfed growth of black spruce, larch, and fir, from the branches of which depends in large quantities a long, light-colored moss, a lichen, containing a nutritious element richer than that of the less developed lichens of the barren grounds. These marshes are the proper *habitat* of the larger or woodland *caribou*, whose physical structure (as will be hereafter seen) admirably adapts it to such treacherous ground.

Its migrations are performed like those of all the migratory Arctic fauna, to the northward in spring, and to the southward in autumn. I am aware that a different opinion is expressed, as to the time of these migrations, by Dr. Richardson, who states upon the information of others only, that "in their migrations to the south, they cross the Nelson and Severn Rivers in May, and to the north in September." If this is true, I think it must be owing to some geographical peculiarities connected with the south-western shore of Hudson's Bay, for I am assured by intelligent correspondents who have passed both winter and summer in the northern portions of the Pine district, that the caribou only makes its appearance within the limits of that district in winter, and Dr. Richardson himself found them during the expedition of 1848, as far north as latitude 69° 40′ in the month of August. These migrations in some cases extend far south of the Pine districts, individuals having been found upon Isle Royale in Lake Superior, and in Aroostook county, Maine. They are not unfrequently seen in the provinces of Newfoundland and New Brunswick, and it is stated, though upon very questionable authority, that the caribou has been found in the northern parts of Vermont and New Hampshire, and among the Adirondack mountains of New York. I have not found any sufficient evidence to show that these migrations ever extend as far south as the forty-fifth parallel of north latitude.

It is rarely seen in that part of the limestone district, south of Peace River, the climate and ground being wholly unsuited to its habits of life.

Dr. Richardson stated in his Journal of 1828, that a larger kind of caribou was found among the Rocky Mountains, but declared that he had seen no specimen of its skin or antlers. In the account of his expedition in 1848 he says, referring to the Rocky Mountains: "Reindeer of a much larger size and darker color than the barren ground variety frequent the mountain valleys." If they exist there at all, their range must be confined to the valleys, for it is obvious that their anatomical structure unfits them for life upon elevated mountain slopes. In New Caledonia, between the mountains and the Pacific coast, it is commonly found, and also along the shores of the Polar Sea and the neighboring islands, from Mackenzie's River to Victoria Land. Its northern limit can be defined with far less accuracy than its southern, but it is well established, that what is called the smaller variety, extends its migrations farther to the north than the other, and the females and fawns of this variety, farther than the males.

It was found by Capt. Sabine on the North Georgian islands and the adjacent mainland in latitude seventy-five degrees north, and he includes it among the nine species of mammiferous animals inhabiting those islands. The skins of a small species of deer, which can be none other than this variety, have often been obtained from the natives of Nootka Sound; and the many readers of the Arctic Journal of the lamented KANE, will not forget the "Bennesoak" that furnished "the glorious meal, such as the compensations of Providence reserve for starving men alone," to the worn down, scurvy stricken crew of the Advance in latitude 78° 40′ on the 23d of February, 1855.

It may be well to state in this connection that the lichens upon which the smaller reindeer feeds, are the *cornicularia tristis* and *divergens*, the *cetraria nivalis*, *cucullata* and *islandica*, and sometimes upon the *gyrophora* or *tripe de roche*, so often resorted to for the support of human life. These are spread over a wide extent of country within the Arctic circle, and are most prized by these animals when the melting snows render them soft and tender. But enough has been said to show that the reindeer is distributed over a wider extent of country than any other species of the American Cervidæ.

Species — Varieties.

I have thus far spoken of two varieties of this ruminant, the "woodland" and the "barren ground," as being distinct from each other. Of the propriety of this distinction Dr. Richardson appears to entertain no doubt. Professor Baird, however, is by no means inspired with the same confidence in this respect, and with the characteristic caution which gives such value to his classifications says, "the opinion is gaining ground, that the barren ground reindeer is distinct from the woodland," and that "it would seem *most probable* that it cannot be the same with the animal inhabiting the circumpolar regions of the old world."

I share to the fullest extent in the doubts of those who hesitate to believe in the existence of two varieties. It is very certain that all the points of difference hitherto pointed out are decidedly unsatisfactory. I am aware that certain anatomical distinctions have been claimed, such as are found in the works of Dr. King, who asserts that the barren-ground species is peculiar in the form of its liver and in not possessing a receptacle for bile, but he regrets that the loss of his collection prevents him from establishing the truth of these assertions.

The first, and most obvious diversity, is said to be in size. The weight of the barren-ground variety is given by Professor Baird at from eighty to one hundred and twenty-five pounds, and by Dr. Richardson at from ninety to one hundred and thirty pounds, exclusive of offal. This weight does not much exceed that of the *Cervus Virginianus*, for I have seen many dressed animals of the latter species, the weight of which much exceeded one hundred and thirty pounds. But in Parry's first voyage, adult specimens of the reindeer were killed, which when dressed weighed only from fifty to sixty pounds, and in this connection what shall be said of the specimen already referred to, which was killed by the hunter Hans, in the Kane expedition, which measured five feet one inch in girth, six feet two inches in length, and stood as large as a two years heifer, the weight of which was estimated at, and from these measurements, obviously, cannot have been less than three hundred pounds, with *the long tuft of white hair depending from the under part of his neck*, giving an appearance of excessive weight to the front view? The animal here described in height, length, weight, and especially in the *white tuft* hanging from the neck, corresponds sin-

gularly with the description hereafter given of an adult specimen of the so-called woodland variety, killed in the month of March, near the pine-wooded banks of the Aux Lievres River. It is nowhere asserted or claimed, I believe, that the woodland variety is found on the Greenlandic peninsula. In fact every description given by Dr. Kane, applies with more propriety to the larger than to the smaller variety, if two such exist.

Captain Parry found individuals on Melville Island weighing one hundred and eighty pounds.

I have not seen the weight of the larger caribou stated, elsewhere than in an article in the new American Encyclopedia, where it is given at from two hundred and fifty to three hundred pounds. What means of arriving at such a conclusion were possessed by the writer of that article, are not given; but from the tenor of the article in other respects, it can hardly be supposed they were extraordinary.

Another point of difference, and one more generally relied on, is said to be found in the thickness, weight, and curvature of the antlers, those of the Arctic variety it is claimed, being much larger, heavier, and more gracefully curved than the other. In Professor Baird's description a wood-cut is given of a right antler from North Greenland. Dr. Richardson gives figures of two, taken from old bucks killed near Fort Enterprise, which differ only from that of Professor Baird in being slightly more palmated. It is also stated that in the woodland caribou, the brow antler is wanting. But there is a pair of antlers in the possession of Professor Leonard Marsh of the University of Vermont, procured by L. G. Bigelow, Esq., from the lumber region before referred to, taken from a buck killed near *Lac de Cerf*, in the interior of a district hundreds of miles from any of the barren grounds, where the smaller variety is not found, which have not only the brow antlers (which form a prominent characteristic of all these cuts, and are shaped like the figures in question), but are longer and curved more gracefully than any of them. Moreover, these antlers exactly fit the skull of the specimen I have described below. I am therefore confident that no difference can be predicated of these two assumed varieties, based upon any comparison of the antlers, and I very much doubt whether any examination will detect others more satisfactory. Upon the evidence as it now stands, I am inclined to adopt the expressions, without by any means acceding to the conclusions, of the last brilliant

transmutation theorist in species, and to claim *that there is but a single species or variety of the American Reindeer*, and that in the "struggle for existence" on the unsheltered barren grounds, within the Arctic circle, unprotected from the severities of an Arctic climate, subsisting upon innutritious food, the animal has degenerated in size and activity; while a more nutritious food, found in greater quantities, the shelter and protection of the wooded swamps of the Eastern district, have made of the so-called woodland variety a "more favored race," a larger and more perfect animal; but that these will not becôme different *species* or even *varieties* any sooner than the runts and fantails of the same author will become transmuted into any thing else than pigeons.

Description.

The following is a description of an adult male, killed on the first day of March 1860, on Lac de Cerf about three miles east of Riviere du Lievre, and about eighty miles north of the confluence of that river with the Ottawa, near the forty-eighth degree of north latitude.

The muzzle is entirely covered with hair, which over the body is long, erect, very compact, and brittle: throat maned. Hoofs, broad and depressed. Accessory hoofs, very fully developed. The external metatarsal gland, above the middle of the leg. Without antlers, but with indications upon the skull of their having been recently cast. Tail, short and depressed. The skull has a large nasal cavity, rather more than half as long as the distance to the first grinder: the intermaxillary moderate, reaching nearly to the nasal.

The exterior color of this specimen varies from pure white to fulvous brown, the prevailing color of the anterior portion being grayish white, and of the posterior, brown. The white color next the skin is invariably clear, the pure white and grayish colors not blending, but having their edges sharply defined. The color of the brown portion is confined to the tips of the hairs, changing to light gray and whitish toward the skin. The edges of the lips (which are fleshy), the muzzle and nostrils, white. The cheeks and upper part of the head, dark brown, growing lighter toward the upper fore neck, which, with the inferior portion of the under jaw, is tawny white. Neck, which is short and muscular, is grayish white, darker above, so that a ridge along the top is a pepper-and-salt color: lower part of the neck

nearly pure white. On the lower part of the neck extending about half the length of it, from the point of the sternum, is a long coarse mane of white hairs, the longest of which measure 3.5 inches. An irregularly shaped patch of white extending from the neck along the middle of each shoulder. The upper parts of the shoulders, back, rump, and flanks, dark fulvous brown, changing to white on the belly, and below and around the anal region. Prevailing color of the legs, light brown. The tail is bushy, and entirely white except a portion of the upper surface an inch and a half in width, and extending from the roots an inch and a half, which is brown. The hairs are flattened. An irregular whitish spot on the inside of each tarsal joint, and a white band about an inch in width, entirely surrounds each leg above the hoof and accessory hoof. The cleft between the phalanges is lined with white hairs. The exterior of the hoofs and accessory hoofs is black, the interior dark gray.

The ears are on the outside dark yellowish brown, somewhat lighter on the inside, covered on both the exterior and interior surfaces with short hair.

The general form of the animal is compact and muscular, having none of the awkwardness of the moose, and but little of the delicate, graceful contour of the *Cervus Virginianus*.

Measurements.

	Inches.
Height at shoulders	52.0
" " rump	52.4
Length from nose to root of tail	83.0
" of tail	4.0
" of hoof from point to union with skin	4.0
" from point of hoof to bottom of cleft between phalanges	8.0
" of accessory hoofs	3.5
Greatest width of accessory hoof	1.5
Length of ulna	11.5
" of humerus	13.0
" from point of posterior hoof to end of phalanges	8.0
" thence along metatarsal to end of *os calcis*	18.0
" of tibia	15.5
Circumference of hoof at union with skin	9.5
" " fore leg at smallest point below knee	5.0
" " " above accessory hoof	10.0
Diameter of spread track	8.0

	Inches.
Length from nose to root of ear	15.0
Height of ears, posteriorly	7.0
" " " anteriorly	6.5
" " " internally, above skull	6.5
Width of nostril	1.4

Measurement of the Antlers above referred to.

Circumference of main stem at the base, seven inches. Two brow antlers, extending from about three inches above the root of the main stems directly forward: length respectively, fourteen and 17.5 inches; each dividing, one at three, the other at eight inches from the base, and each prong again subdivided within about four inches of the end.

Width of brow antler at widest part, three inches. The two main branches start from the main stem four inches above the brow antlers, and are each twenty-three inches long, extending forward. These are again divided and subdivided. Their circumference at the base, eight inches; width of widest part of main stem, 5.5 inches. Number of points from each stem aside from brow antler and main branch, five; length of longest, four inches. Length of main stem, forty-three inches; distance between tips, seventeen inches. Greatest distance between main stems, twenty-seven inches. Greatest deflection in curve of main stems from tip to base, 11.5 inches.

Another specimen, killed near the same place on the 15th of February, 1860, is a female, and much smaller. The color, general aspect, and specific characters the same.

Morphological and Physiological Peculiarities.

The width, size, and general position of the *brow antlers*, as they are termed, distinguish this deer from any of the other cervidæ; while the cleft hoofs and broad accessory hoofs, with the depressed character of the hoofs themselves, together presenting a surface to support the weight of the animal equivalent to a circle, the diameter of which is not less than eight inches, form another peculiarity of the species, enabling the individual to support its weight on the crust of deep snows, or on the soft ground of the morasses.

Among other noticeable structures is a *trachea* of very extraordinary diameter, with a respiratory apparatus of great power; this, joined to a remarkable muscular development, particularly at the shoulders and haunches, must give to the Caribou great speed and endurance. An intelligent observer, Mr. R. J. Lusk, of Ottawa City, writes that "their speed is so great, that they cannot be run down at any season of the year," and that "Indians have followed herds of them for many days in succession, and then found themselves farther from the herd than when they started. The Indians all agree that they cannot be taken except by stealth." He refers, also, to an instance where a staghound which could catch the common deer with ease, was started after a herd of nineteen on Lac de Cerf, when the herd ran away from him so quickly that he seemed to be almost stationary. He gave chase all day, the Caribou running from lake to lake, but in the evening gave it

up. The pelage is erect and very compact, and the skins form an article of clothing better adapted than any other for Arctic travelling, and afford an adequate protection for the animal against the intense severity of winter within the Arctic circle. I have not been able to discover the "cist or bag in the lower part of the neck, about the bigness of a crown piece, and filled with fine flaxen hair, neatly coiled around to the thickness of an inch," described by Mr. Hutchins, and I am sceptical as to its existence. The form of the antlers varies so much in different individuals, that nothing can certainly be determined from them in regard to sex or age. Their greatest growth is unquestionably produced when the animal is in its most vigorous condition, and I see no reason to question the statements of those who speak of their weight as sometimes reaching forty or fifty pounds.

When in condition, there is a layer of fat deposited on the back and rump of the males, to the depth of two or three inches or more, immediately under the skin, which is termed *depouillé* by the Canadian voyageurs, and as an article of Indian traffic it is often of more value than all the remainder of the body. This is thickest at the commencement of the rutting season; it then becomes of a red color and acquires a high flavor, and soon afterwards disappears. The females at that period are lean, but in the course of the winter they acquire a small *depouillé*, which is exhausted soon after they drop their young. The uses of the Caribou to the Indians of the barren ground, are almost as numerous as those of the Reindeer in Lapland. The flesh, which, in condition, surpasses the finest venison in flavor, is eaten separately or enters largely into the manufacture of pemmican, the staple food of the Arctic zone. Of its horns are made fish spears and hooks, as well as ice chisels and other tools, and the skin dressed with the fur supplies the place of both blanket and bed to the inhabitants of the Arctic wilds. When properly dressed, it makes an excellent leather, and the skins sewed together are used for making tents. The shin bone, split so as to present a sharp edge, is used as a knife. The undressed hide is cut into thongs which are applied to many uses, and a very good article of sewing thread is made by splitting the tendons of its dorsal muscles.

Habits.

The Caribou is the only American deer that can with much propriety be called gregarious. Though the moose and common red deer are sometimes found in "yards," as they are termed, in winter, they collect in this manner more for the purpose of mutual protection from severe cold and for convenience of procuring food than from any natural disposition thus to flock together. The larger Reindeer are usually found in herds of from six to twenty, but the smaller herd in much larger numbers, sometimes as many as several hundreds. When run with dogs, the herd rarely, if ever, separates. Very singular modes of capturing them are said to be put in use by the Chipewyan, Esquimaux, and Dog Rib Indians. They are very inquisitive animals, and if they see any suspicious object that is not actually chasing them, they will gradually, and after forming repeated circles, approach it. The hunter, if there is a rock convenient, cautiously creeps behind it, and imitates the bellow of the deer when calling to each other. If he has sufficient patience he may be sure the animals will ultimately come to examine him. The Esquimaux rarely shoots until he is within twelve paces of his game. When two hunt in company, they show themselves to the deer and then walk slowly away, one before the other. The deer follows, and when the hunters come near a stone, the foremost drops behind it, while his companion walks steadily forward. The deer follows after the latter, and thus is brought within range of the concealed man. Often, also, when assembled in herds, they are driven into the sea, through *passes*, where, swimming about, they are speared in great numbers by the Indians in their kayacks. They are also taken in pit traps made from snow and covered with lichens. A very interesting account of various other methods of hunting and capturing them is given by Dr. Richardson in his account of the North American Fauna, which it is unnecessary here to repeat.

Is the American Species identical with the European?

No one who has carefully studied the animals of the northern portion of continental Europe, can fail to have been impressed with the remarkable correspondence of the species there found with those in Arctic America. This is particularly true of those comprised in the

Scandinavian Fauna, corresponding species being almost invariably found in North America. These will be found to differ very slightly in size, color, and less important particulars, but never sufficiently upon the most liberal basis of classification to be reckoned as different varieties even of the same species. I am not disposed to believe that the Reindeer forms any exception to this marked coincidence of species, although I have not had the means of comparing the American and European together, which must now be regarded as almost the only conclusive or satisfactory method of determining questions of species. I can detect no material difference, except in color, between the figure given in "*La Regne Animal*" and American specimens which I have examined, and as that is the figure of one kept in a menagerie, that fact alone is enough to account for any difference in color. The habits and modes of life of the two, are no doubt widely different, but whether this difference is to be attributed to any other cause than that of "variation under domestication" is a question that must be determined hereafter, with the other problems connected with that subject.

NOTE.—Since the foregoing article was completed, my attention has been directed to the following passage in Prof. Baird's account of the Cervus Virginianus. If the fact there stated in relation to the difference in size be true, how much more probable it is that the same species of Reindeer, subsisting under conditions of life much more diverse than those of the northern and southern specimens of the C. Virginianus, spoken of by Prof. Baird, should also differ in size and weight as I claim they do? Professor Baird says: "It is a well-ascertained fact that the more northern specimens of this deer are appreciably larger than those found further south, and that the deer of the southern sea-coast and its islands are smaller than those of the uplands and mountains of the same latitude. This fact is well established by the comparison of specimens in the Smithsonian collection from New York and from St. Simon's Island."—*Mammals of the Pacific R. R. Survey*, p. 648.

IV. PHYSIOLOGY.

1. SOME EXPERIMENTS AND INFERENCES IN REGARD TO BINOCULAR VISION. By Prof. WILLIAM B. ROGERS, of Boston, Massachusetts.

IN the theory of binocular vision which has been so ably expounded by Sir David Brewster, Brücke, and others, it is contended that no

part of an object is seen single and distinctly, but that to which the optic axes are for the moment directed, and that "the unity of the perception is obtained by the rapid survey which the eyes take of every part of the object." According to this view our perception of an object in its solidity and relief, instead of being the simple and direct result of the pictures formed at any one moment in the two eyes, is acquired by a cumulative process, in which the optic axes are successively converged upon every point of the object within view.

In an experimental discussion of some points in binocular vision published in the American Journal of Science several years ago,* I was led to conclude that the phenomena of the stereoscopic resultant do not necessarily or even usually conform to these conditions, and that "the perception of a perspective resultant line or of a physical line in the same attitude does not require the successive convergence of the axes to *every point.*" The grounds of this conclusion were,— first, that the perspectiveness of the resultant although not perceived when the axes are steadily maintained at any one convergence, appears as soon as they are allowed sufficient freedom of motion *to unite a few contiguous points* of the component lines, and that it then affects the whole extent of the resultant; and second, that the resultant presents a perspective attitude even when the component lines, instead of being united into one, are brought together so as to intersect at a small angle, *each of the intersecting lines in this case appearing in relief.*

Satisfied from these considerations that the perceived singleness and relief of the resultant are to be ascribed rather to a *process of suggestion* than to the exhaustive binocular survey which has been supposed necessary, I was led to the opinion that, while some change of direction and convergence of the axes is needed for the effect, the successive view of a few contiguous points is all that is required to bring the suggestive action into play and to give rise to the full perception of the position and relief of the resultant.

In this view while rejecting the theory of successive vision in the form in which it has been propounded, I still considered some degree of motion of the axes as one of the steps by which we obtain the perception of the binocular resultant.

The following experiments, intended still further to test the theory

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* Volume XX. pp. 86, 204, 318, and XXI. pp. 80, 173, and 439.

of the successive combination of corresponding points in binocular vision, are believed to be in part new, and are in part modified repetitions of experiments already described by Profs. Wheatstone and Dove. They offer what seems to be decisive proof that such a successive combination of pictures point by point, however it may enter in many cases into the complex process of vision, *cannot be regarded as an essential condition to the singleness and perspectiveness of the binocular perception.*

1. Let a brilliant line, held in a perspective position at a convenient distance midway between the eyes, be regarded intently for a few seconds so as to produce a lasting impression on the retinæ. On turning the eyes towards a blank wall or screen the subjective impression will be seen projected against it, and having the same perspective altitude as the original line. If, then, one eye be closed, the line will appear to subside into the surface of the screen, taking an inclined position corresponding to the optical projection of the original line as seen by the unclosed eye, and therefore corresponding to the image formed in that eye. By opening and closing the eyes alternately, and finally directing both to the screen, we are able to see the two oblique lines corresponding to these projections and their binocular resultant corresponding to the original object.

2. Let two slightly inclined luminous lines formed by narrow slits in a strip of black card-board be combined into a perspective resultant either with or without a stereoscope. Looking at this for a few seconds so as to induce the reverse ocular spectrum, and then directing the eyes towards the opposite wall of the apartment, *a single spectrum will be observed having the attitude and relief of the original binocular resultant.*

As a strong illumination of the lines is necessary to bring out the most striking effect, the card-board should be held between the eyes and some brilliantly white surface, as the globe of a solar lamp or a strongly illuminated cloud, care being taken to prevent the entrance of extraneous light.

3. Using the same arrangement, let the luminous lines be regarded in succession each by the corresponding eye, the other eye being shaded so that no direct binocular combination can be formed. On looking toward the wall, it will be seen that *the two subjective images unite to form a single spectral line, having the same relief as if the lines had been directly combined* with or without the stereoscope.

While the perspective image continues distinctly visible, let either eye be closed, the other being still directed towards the wall. The image will instantly lose its relief and take its position on the plane of the wall as an inclined line corresponding to the subjective image in the eye which has remained open. When the subjective impressions have been sufficiently strong, it is easy to alternate these effects by projecting first the picture proper to the right eye and then that of the left on the plane of the wall, with their respective contrary inclinations. On then looking with both eyes we see the resultant image instantly start forth in its perspective altitude.

It is hardly necessary to say, that in order to obtain these effects satisfactorily even with lines very strongly illuminated, the observer should have some practice in experiments on subjective vision. *In these circumstances, however, I have found the results to be perfectly certain and uniform.*

The conditions of the experiments are obviously such as to exclude all opportunity of *a shifting of the image on the retina.* Such a shifting, however, is essential to that successive combination of pairs of points in the two images which, on the theory of Brewster, is required for the production of the single perspective resultant. Hence, according to this theory, the resultant spectrum in these experiments, instead of being a single line in a perspective position, ought to present the form of two lines inclined or crossing, situated in the plane of the wall without projection or relief.

In reference to the first two experiments, it might perhaps be maintained that as the perspectiveness of the original line or resultant on which the eyes were converged formed part of the direct perception in the first stage of the experiment, it would be likely through association to be included also in the spectral or subjective perception. But this consideration, which at best does not impress me as of much weight, is entirely inapplicable to the conditions of the last experiment, where the eyes are in the first place impressed *in succession* with their respective images, and where yet when they are together directed to the wall, *the perspective single resultant at once springs into view.*

4. Without resorting to these troublesome efforts of subjective vision, the following is a simple proof that pictures *successively* impressed on the respective eyes are sufficient for the stereoscopic effect. Let a screen be made to vibrate or revolve somewhat rapidly between the eyes and

the twin pictures of a stereoscope, so as alternately to expose and cover each, *completely excluding the simultaneous vision of the two.* The stereoscopic relief will be as apparent in these conditions as when the moving screen is withdrawn.

Here there is no opportunity for the combination of pairs of corresponding points in the two diagrams by the simultaneous convergence of the optic axes through them, but at each moment the actual picture in the one eye, and the retained impression in the other, form the elements of the perspective resultant which we perceive.

5. The ingenious experiments described by Prof. Dove many years ago in which the stereoscopic effect was obtained by the momentary illumination of the electric flash, furnish a further and most powerful argument against the theory of successive binocular combination here referred to.

In repeating these I have found great advantage in using one of Ritchie's improved Ruhmkorff coils, having a coated jar included in the outer circuit, the intensely brilliant spark of which can be made to throw its light upon the object viewed in any direction or at any interval that may be desired.

When a twin diagram of any simple geometrical solid was placed in the stereoscope and viewed by this momentary light, it was found to exhibit the perspective resultant in most cases with a single spark, and it never failed to present it in perfection with a succession of sparks even when they followed each other slowly.

A large circular disc of brass, marked with the usual concentric striæ, was placed in a position to catch the illumination and produce the peculiar intersecting lines of reflected light. At each spark the bright resultant line due to the binocular union of these intersecting lines was seen penetrating the disc, and extending in a steep angle beyond and in front of it.

As, according to Wheatstone, the duration of an electric spark is less than one millionth of a second, it can hardly be supposed that in either of these experiments the eyes have time to make the successive changes of direction required, by the theory, for the singleness and relief of the observed resultant. Not less at variance with this theory is the familiar fact that the illumination of a single flash of lightning is sufficient to give us a clear perception of the forms and positions of objects to which the eyes are for the moment directed. So the long straight spark of

one of Geissler's narrow vacuum tubes, glowing for an instant in a dark room, impresses a precise perception of the altitude and place of the tube and its included line of light; and even the irregular path of the long spark through the air produces a distinct perception single and faithful to its devious directions.

We may therefore conclude, *first*, that the perception of an object in its proper relief, or that of the perspective resultant through binocular combination in a stereoscope, or otherwise, may, and most usually does, arise, by *direct suggestion* from the two pictures impressed, without requiring the successive combination of corresponding points;—*and second*, that for the singleness of the resultant perception, it is not necessary that the images should fall on what are called corresponding parts of the two retinas.

The condition of single vision in such cases seems to be simply this, that the pictures in the two eyes shall be such and so placed as to be identical with the pictures which the real object would make at a given distance and in a given altitude before the eyes.

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2. On our Inability from the Retinal Impression alone to determine which Retina is impressed. By Professor William B. Rogers, of Boston, Massachusetts.

Although on first view it might be supposed that an impression made in either eye must necessarily be accompanied by a mental reference to the particular organ impressed, it will be seen from the following simple experiments that the impression of itself is not essentially suggestive of the special retinal surface on which it is received.

Exp. 1. Let a short tube of black pasteboard one fifth of an inch in diameter be fixed in a hole in the centre of a large sheet of the same material. Hold the sheet a few inches before the face of a second person, and between him and a bright window, moving it to and fro until the bright circular aperture of the tube is brought directly in front of one of the eyes, suppose the left eye; and let him fix his attention upon the sky or cloud to which the tube is directed. He

will feel as if the impression or image of the hole *belongs equally to both eyes* and will be unable to determine which of them really receives the light.

On moving the aperture towards the right, or nearer the nose, but not so far as to be out of the view of the left eye, or to be visible by the right, the observer will imagine that it is now in front of the right eye and chiefly seen by it. Shifting it still further in the same direction, until it is brought within view of the right eye, but not fairly in front of it, it will appear as if placed before the left eye, and by an additional motion bringing it fairly in front of the right eye it will seem to be equally before both eyes, or to be in the medial line between them.

Like effects may be observed by using a half sheet of rather stiff foolscap with a large pin hole in the centre. Bending this over the face and moving it until the hole is in front of one of the eyes, the same uncertainty and contradiction will be produced as in the preceding experiment.

Exp. 2. Similar results may be obtained by rolling half a sheet of letter paper into a tube of about one inch in diameter, and holding it before and a little in advance of one eye while both are directed to a white wall some feet distant. Keeping the view *fixed* upon the wall there will be seen upon its surface a circular image of the remote aperture of the tube. This, as we look intently at it, will appear as if seen equally by both eyes, occupying a midway position between them. If now the eyes be converged to some point nearer than the end of the tube the circular image will appear against the side of the tube giving the impression that it is seen by the eye which is remote from the tube and is at the same time directed toward the outside. For the complete success of this experiment the wall should be only moderately bright, and but little light should fall on the exterior of the tube next the uncovered eye.

Exp. 3. Let two tubes of stiff paper, each one inch in diameter and six inches long, be held close to the two eyes in a converging direction so that the outer ends may touch each other. Then directing the view through them to a white wall at a short distance, the observer will see the two tubes as one, with a single circular opening clearly marked out on the wall. If now a small object as the end of the little finger be brought near and in front of one of the tubes, it will take its

place within this circle and will seem to be equally an object of vision to both eyes, so that the observer will be wholly unable to decide before which eye it is actually placed.

Let the observer next direct his view to a very remote object, as the sky, seen through the window, still retaining the previous adjustment of the tubes. He will now see two circles, continuing separate as long as he keeps his eyes fixed on the distant surface; and if the finger be held up as before in front of one of the tubes it will appear within the circle which is in front of *the other eye;* thus causing the impression on the right eye to be apparently transposed to the left, and vice versa.

Exp. 4. Fasten a small disc of white paper on a slip of black pasteboard of the size suitable for a stereoscope, and place this in the instrument so that the white spot shall be centrally in front of one of the glasses.

To a person not aware of the position of the spot it will appear in the stereoscope as if equally in view to both eyes, and he will be entirely unable to decide on which retina its picture is impressed. Indeed, properly considered, the spot does not appear directly in front of either eye, but is seen at the intersection of the optic axes, in the medial or binocular direction between the two.

Let the spot be now moved toward the right side but still within the range of the left eye, and it will seem to be before the right eye rather than the left. Shift it into the right compartment, but not far from the dividing line, and it will appear as if seen chiefly by the left eye; and, finally, bring it to the middle of the right compartment and it will seem as at first to belong equally to both eyes.*

Referring to the results observed in the above experiments when the object is directly in front of either eye, it may be concluded that the mere retinal impression on either eye is unaccompanied by any consciousness of the special surface impressed, and that the formation of the visual perception appertains to that part of the optical apparatus near or within the brain, *which belongs in common to both eyes.*

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* The effect here described is one of a series of phenomena which Dr. O. W. Holmes attributes to an *actual transfer* of impressions from one eye to the other, and which he proposes to explain by the hypothesis of reflex vision. Proc. Amer. Acad. Arts and Sciences, Feb. 1860.

These observations show, moreover, that the *perceived direction is just as truly normal to the central part of the retina which has received no light, as to that of the retina on which the white spot has been painted.* Indeed, as before indicated, it is normal to neither, but is felt to be in the middle line between the two, that is, in the binocular direction. It need scarcely be added that this conclusion is at variance with the law of visible direction maintained by Brewster, which requires that the apparent direction of an object shall in all cases be normal to the part of the retina impressed.

The reference of the object in certain cases above noticed (parts 1, 2, and 4) to *one eye chiefly*, and that the eye from which *it is actually hidden*, is accounted for by the direction in which the other eye receives the light. As this direction, in the case of the left eye for instance, would be decidedly toward the field of view of the right eye, it would at once suggest the place of the object as somewhere before that eye, and so when the object is actually before the right eye, but in a position towards the left, it would excite the idea of an object somewhere before the left eye. As the retinal picture alone gives no indication of the particular eye in which it is formed, but only excites a visual consciousness common to both, the object in these cases will seem to be visible by both eyes, but chiefly by that before which the suggestion just mentioned would naturally place it.

A like explanation applies to the transposition observed in Exp. 3, when the view is directed to a distance through the converging tubes. Here the false visual reference of the finger depends on the fact that the circle in front of either eye is suggestive, merely by its position, of a *special vision by that eye*, while from the conditions of the experiment these circles are in fact reversed in their places as compared with the tubes and eyes to which they appertain.

We have seen in the above experiments that when an object is presented to one eye without any accompanying circumstances leading us to refer the visual act specially to this or to the other eye, we have a consciousness of seeing it equally with both eyes. *The same result occurs when separate objects are presented to the two eyes*, provided as before, extraneous sources of suggestion are excluded.

Exp. 5. Thus if we place on the black slide of the stereoscope two spots, differing either in shape or color, one before each eye, we perceive them both in the middle or binocular direction, each seemingly visible

in an equal degree to both eyes, the one being seen through or upon the other according to the fitful attention or suggestion of the moment. A pleasing modification of this experiment is made by using two unequal white spots on the black slide and interposing a green or other colored glass between one of them and the lens. The spot which appears colored will give as strongly the impression of being seen by both eyes as the white one, in spite of our knowledge of the position of the colored glass.

Even in cases where the two objects are wholly unlike, and at very different distances from the eyes by which they are severally regarded, this feeling of a common or united visual act in regard to each of them is often easily recognized. Of this we have a ready illustration in the familiar experiment on ocular parallax in which a distant object, hidden from one eye by an interposed finger or pencil, is seen through or behind the pencil when both eyes are directed towards the distant object.

Exp. 6. To observe this effect satisfactorily, it is well to make the experiment in an apartment in which a single small lamp is placed at some distance from the spot on which we stand. Looking intently at the lamp, we bring the pencil before the face in such position as to give us an image on each side of the lamp, and then move the pencil toward the right until its left hand image seems to coincide in direction and position with the lamp, which appears to shine through or to partially replace it. As we continue to look thus at the lamp, we have a clear impression that both lamp and pencil are equally visible to both eyes, and without some consideration of the previous adjustment and motions we are unable to determine which is actually visible to the right and which to the left eye.

The same experiment furnishes also an incidental illustration of the principle of *transposed* visual reference before alluded to. If, while the above adjustment is maintained, we contemplate the other image of the pencil situated some distance to the right of the lamp, and endeavor to decide, from the mere visual impression, to which eye it appertains, we almost unfailingly refer it to the right eye as that which most nearly fronts it, although obviously it belongs to the other, as will be found at once on closing either eye.

Where the eyes are externally very sensitive, any strong illumination of one as compared with the other will interfere with the effect above

described by referring the impression specially to the eye thus unduly excited. In such cases the observation is best made in a moderately lighted room by interposing the pencil between the eye and a vertical stripe on the wall.

Exp. 7. Recurring to Exp. 2, in which with a tube in front of one eye we perceive a bright circle on the wall in the medial direction, we may obtain a pleasing illustration of the point now under consideration, by bringing a dark card or book or even the hand between the uncovered eye and the wall. The spot instead of being intercepted will appear as a perforation in the opaque screen.

Here, as in the case of the pencil and lamp, the bright circle and the screen are both optically referred to the intersection of the two lines of view. But the luminous circle almost or entirely obliterates the corresponding part of the screen. As the full view of the screen and its connections continually reminds us that it is in front of the uncovered eye, we are led to refer the luminous circle seen as coincident with a part of it, to the same eye, and thus to believe that *we are looking through the screen with that eye.* It is, however, not difficult, by intently regarding the luminous circle, so to counteract the force of this extraneous suggestion as to feel even in this case as if the circle were equally in view to both eyes.

These considerations explain very simply the experiment of the pseudo-diascope described by Mr. Ward of Manchester, which like several of those above mentioned is but an instance of the old observation of Da Vinci, that when we see behind a small opaque object presented near the eyes, "it becomes as it were transparent." In making this experiment with a tube of paper supported between the thumb and forefinger of the left hand, and held before the right eye so that the back of the hand may be some inches in advance of the left eye, it will be noticed that the effect varies with the amount of convergence of the eyes, and that the bright perforation in the hand *may or may not* be referred to the left eye according to the force of the accessory suggestions or the intentness with which we fix our gaze upon the distant spot to which the axes are converged.

In conclusion it may be remarked, that the experiments which have been described are for the most part too obvious and familiar to have merited such a special notice but for the peculiar and in some respects new interpretation which they have offered of many visual phenomena.

Considered in this relation we are, I think, entitled to conclude from them: —

First, that *the retinal impression of an object presented directly to either eye is accompanied by the feeling of a united visual act, and of itself gives no indication of the particular eye impressed;* and,

Second, that *the reference of the impression to one eye rather than the other is the result of collateral suggestion, which may either locate the image in the eye that actually receives it, or may transpose it seemingly to the other, according to the particular conditions of the observation.*

C. PRACTICAL MECHANICS.

I. ENGINEERING.

1. Observations on Hydraulic Cements. By Lieutenant Q. A. Gillmore, Corps of Engineers, U. S. A.

Having been engaged for the last two years in conducting a series of experiments with the hydraulic limes and cements from different geological and geographical localities in the United States, with a view to ascertain the relative value of the mortars derived from them, as regards hardness, adhesiveness, hydraulic energy and other coördinate properties, it has been suggested to me, that a few observations upon any points, hitherto not fully understood, or considered doubtful, might be of interest.

The following notes have been selected from a mass of manuscripts, which is intended to furnish the basis of a preliminary report to the chief engineer of the war department.

The nature and properties of calcareous cements, and their correct mode of manipulation and treatment present a wide field for experimental research. The attention of chemists and practical engineers has, within the last half century, been turned in this direction in an especial manner, and the writings of Vicat, Pasley, Rancourt, Treussart, Kuhlman, Chatoney, Rivot, and Gen. Totten (our own chief engineer), have thrown a flood of light upon the subject. It is not to be concealed, however, that many phenomena are developed in the preparation and manipulation of calcareous mortars, from the time the raw stone enters the kiln for calcination, until it has attained its final resting-place in the state of mortar, and is undergoing the hardening process, that are even now shrouded in comparative mystery, a mystery indeed, against which the amplest resources of the analytical chemist, have been brought into requisition in vain. Until within a very recent

period, for example, the cause of the hardening or "setting" of hydraulic mortar was only a subject of vague conjecture which developed itself into a variety of speculative and contradictory theories. Even now, there is a remarkable diversity of opinion upon this point, which laborious and patient research only can dissipate.

It is now very generally conceded that, when ordinary limestones, or the purer marbles, are burnt in kilns, they part with their water of crystallization and most of their carbonic acid, assume the amorphous state, become highly caustic, have a great avidity for water, and are very light and spongy; that, when these burnt limes are immersed in water, a quantity of the liquid equal to nearly one quarter of their weight, is rapidly absorbed, and the hydrate of lime is formed. It is also generally believed that ordinary mortar (a mixture of a paste of the hydrate of lime and sand), hardens or *sets* in consequence of the absorption of carbonic acid from the air aided by desiccation. By some writers it is claimed that the sand, if silicious, is not entirely inert, but is acted upon by the lime, when an insoluble silicate of lime is formed. This, however, is somewhat doubtful. At all events, it does not take place, if the mortar is placed under water, or in close vessels, as it is well known that it will remain in a state of paste for an indefinite period under such circumstances.

When we come to consider those hydraulic limes, however, which contain from ten to thirty per cent. of clay, and the hydraulic cements which contain from forty to sixty per cent. of that material, or another distinct class of impure limestones which seem to owe their hydraulic energy to the presence of a notable quantity of carbonate of magnesia, frequently not accompanied by clay at all, we must seek for some other explanation of the hardening process; for it is well known that hydraulic mortars will set under the exhausted receiver of an air-pump, and that, when immersed in water or kept constantly wet, they are considered in the most advantageous position for attaining their maximum strength and hardness. The setting of hydraulic mixtures, therefore, would seem to be due to a compound action; else a large proportion of the ingredients enter as inert matter, or are absolutely hurtful in their effects. By some, it was formerly supposed that the oxide of iron, by others, that the oxide of manganese was capable of conferring hydraulic energy. Smeaton suggested that this action was due to the presence of clay in the limestone. Subsequently, Vicat enlarged this idea into a

comprehensive theory, which was finally applied in practice on an extensive scale, in the manufacture of artificial hydraulic lime, by mixing unburnt clay with common lime, in suitable proportions, and calcining them. The discovery of the hydraulic character of the dolomites, and those impure magnesian limestones which contain very little clay, rendered a modification or enlargement of Vicat's theory necessary. It was also conjectured that the presence of potash or soda might exercise an influence in the hardening process. Although *ordinary* chemical analysis failed to detect either of these substances in the hydraulic limestones, a variety of phenomena indicated their presence in quantities quite sufficient to produce marked chemical reactions. There seems to be little doubt indeed, that the alkaline silicates or carbonates, although not invariably present in *common* limestones, are always to be found in those argillaceous and argillo-magnesian limestones, of whatever geological epoch, that have been found to be most suitable for hydraulic purposes, and it has even been claimed by Kuhlman, that the hydraulic energy is due in a great measure to their action. It is known that, if a solution of the silicate of soda or of potash be mixed with common lime, the soda or the potash will be set free, and the silicic acid will enter into combination with the lime, forming an insoluble silicate of lime, a basic silicate, in fact, which, upon exposure to the air, absorbs carbonic acid, and is converted, gradually, into what might be termed silico-carbonate of lime. This, in time, acquires great hardness. The alkalies, in this case, act simply as carriers of the silica, to present it in a useful form to the hydrate of lime.

There would seem to be little doubt that their useful effect has been overrated. The proportion in which they enter the American cements is usually quite small, the most careful analyses made specially with reference to them having failed to detect as much as one per cent. It is also probable, although the statement is advanced with some misgivings as to its entire correctness, that their action is limited strictly to hastening the *commencement* of solidification, and that the advantage to be derived from this circumstance is more than counterbalanced by the presence of unsolidified caustic potash or soda, the tendency of which would be to diminish the strength and ultimate specific gravity of the mass.

It seems highly probable that the principal and perhaps the only really efficient cause of the hardening of hydraulic mortars is to be

found in the formation of hydrated double silicates of lime and alumina, or of lime and magnesia, depending on whether the limestones are argillaceous or magnesian in character. In mortars derived from argillo-magnesian limestones, both of these compounds would be formed. This theory, although accounting for most, is not logically applicable to all the phenomena attending the setting of hydraulic mixtures. It does not clearly explain why, among several cements of identical composition, according to a careful analysis, some will set with great rapidity in water, others very slowly, and others again, not at all; or why some will be more affected by a change of temperature than others; or why, in some, the hydraulic action will be quickened, and in others retarded, by the addition of hydrate of common lime; these and a variety of other interesting facts, patent to any experimenter of ordinary sagacity, are still enveloped in doubt and uncertainty. For a full and complete solution of these questions, we must look to those who, to a judicious combination of theory and practice, are willing to superadd a patient, persevering, and zealous industry.

The oldest limestone capable of furnishing hydraulic cement *overlies* the Potsdam sandstone at the base of the Lower Silurian system. This sandstone is very intimately related to the calcareous beds above. It is known as the calciferous sand rock, and as its name indicates is calcareo-silicious in character.

Three distinct masses of this rock are usually observed, wherever it presents a fully developed outcrop, the lowest of which ordinarily contains a marked excess of silica, while those above more nearly resemble the purer limestones which overlie them, and into which they finally pass with certain alternations of argillaceous and slaty matter. All the deposits which lie above the Potsdam sandstone and below the Utica slate or its corresponding member, are classified by Prof. Rogers in his formation No. II. Among them, sometimes higher and sometimes lower in the series, occur numerous beds of argillo-magnesian limestone possessing hydraulic properties in an eminent degree.

After leaving the limestones of Formation II. in the ascending series, we pass up through the equivalent of the lead-bearing strata of Wisconsin, Iowa, and Illinois, the blue limestones and marls of Ohio, and the Hudson River and Clinton groups of New York, without meeting with any deposit suitable for hydraulic lime or cement, until we reach the Niagara group, in the beds of passage between the shale and

limestone of that group. This underlies in regular order the Onondaga Salt Group of the New York Survey (which appears to be wanting in the West), and has been burnt for cement in Monroe county, and other places in New York.

Still higher up the series, among the top layers of the Onondaga Salt Group, called generally the "Magnesian Deposit" of that group, we encounter another calcareous deposit possessing hydraulic properties, and overlying this the *water lime group* proper, from which the celebrated Rosendale cement from Ulster county, New York, is manufactured.

This deposit furnishes at least nine tenths of all the cement made in the United States, and is claimed by the manufacturers to produce an article superior in quality to all other native cements, and equal in every essential requisite to the so-called Parker's Roman cement. It is an argillo-magnesian limestone belonging to the tentaculite or water limestone of the lower Helderberg group, and is found within a narrow belt, scarcely exceeding a mile in width, skirting the northern base of the Shawangunk mountains. It is not, however, confined to this locality, but can be traced in a southwesterly direction through Ulster and Sullivan counties to the New York State line at Carpenter's Point, thence in a narrow strip along the left bank of the Delaware River to Walpack's bend, where it crosses into the State of Pennsylvania. In a northerly direction, it has not been distinctly recognized east of the Hudson River, but at the mouth of Rondout Creek, takes a turn due north, and can be traced along the west bank of the Hudson, a distance of five or six miles, with occasional glimpses of it still higher up. Except in Ulster county, towards the northern terminus of this range, this particular stone has not been manufactured into hydraulic cement, and has not, in fact, been very critically examined with that view.

The deposit is found occupying every conceivable inclination to the horizon, the direction of the dip being usually northwest or southeast. The entire face of the country in this neighborhood exhibits evidences of a succession of remarkable upheavings, some of them having taken place while the limestone was yet in a plastic form, whereby the strata were twisted into a variety of tortuous and complex shapes; while others, transpiring at subsequent periods, more or less remote, have ruptured the beds, producing a multitude of seams, running diagonally across the lines of stratification.

The useful effect of these upheavings has therefore been to develop into accessible and convenient positions, a vast amount of the cement stone that would otherwise have been buried beyond the practicable reach of ordinary mechanical skill.

The aggregate thickness of this deposit, including certain ledges of other rocks of from four to six feet thick, which interlie the cement strata, averages about forty-six feet.

It is subdivided into several distinct layers, widely dissimilar, as a general thing, in the color, grain, and texture of the raw stone, as well as in their hydraulic properties after calcination. No one manufacturer makes use of all of these cement strata, and no two of them, of the same strata in the same proportions. This is due, *principally*, to changes, or variations, within comparatively short distances, in the hydraulic properties of stone from the same stratum, a striking and characteristic feature of these deposits. In some localities, moreover, the upper strata of the cement-bearing series have been entirely removed by abrasion, while in others the lower ones have been thrust so much out of place by the interposition of other rocks, or are so far below the general surface level, that they cannot be reached with facility or economy.

Few of the manufacturers have rendered themselves familiar with the distinctive characteristic properties of the several layers of stone which they introduce into their combination, instances being comparatively rare, where they have caused them to be quarried, burnt, and ground separately, even for the purpose of experiment. With some exceptions, all the stone taken from a quarry enters into the cement.

This includes certain layers of little or no value when used alone, and incapable of furnishing any thing better than an inferior hydraulic lime, which will not harden under water, if immersed in the state of paste, and frequently, if allowed to set in the air, will not remain hard after immersion, but swells up, softens and falls to pieces, assuming either the consistency of a spongy paste, or of a granulated mass entirely devoid of cohesion.

Although mortars giving rise to such phenomena, evidently contain a large excess of caustic lime, not susceptible of neutralization by the silica, alumina, and magnesia which accompany it, in the formation of hydrated double or triple silicates which are practically insoluble in water, it does not necessarily follow that their incorporation into the aggregate product of a quarry is absolutely injurious.

In some cases, their introduction is believed to be beneficial, when their province appears to be to furnish those requisite constituent ingredients of good cement, either not to be found in sufficient quantities in the contiguous rock, or in proportions capable of considerable increase, without producing an injurious excess.

The cement works in Ulster county are scattered along a line of fifteen miles in extent, those furthest from the Hudson being at High Falls, twelve miles from the mouth of Rondout Creek.

In general terms, the method pursued by the manufacturers is briefly as follows: The stone, after being quarried, is broken into pieces not exceeding eight or ten inches in their largest dimensions. It is then burnt in a cylindrical kiln, terminated at the bottom by the inverted frustrum of a cone. The fuel employed in the calcination is anthracite coal, broken up very fine. What is known as "pea and dust," sometimes called the "second screenings," at the coal-mines, has been found most suitable for that purpose. It is mixed with the raw stone, in the kiln, in alternate layers. When once in operation, the kiln is perpetual, the burnt stone being drawn from the bottom once or twice every twenty-four hours, at which times raw stone and fuel are added at the top in suitable proportions.

There are serious defects in this method of burning, for which no easy and practicable remedy has yet been discovered. The compound character of the cement stone renders its fusion comparatively easy, at a temperature not much above that practically necessary for its calcination. Some of the stone, therefore, becomes so much overburned (having reached the stage of incipient vitrification), as to be not only useless for cement, but exceedingly difficult to reduce to powder, while another portion, usually the largest fragments, or those that have accidentally subsided too rapidly in the drawing, are underburnt, and perhaps partially raw inside. These being also difficult to grind, are excluded, for that reason, if for no other, and are usually subjected to a second calcination. Lying between these two extremes, we find the properly burnt stone; we also find other portions not distinguishable from the latter in general appearance, and frequently forming an important proportion of the whole, some parts of which have been subjected to a complete, and others to a supercalcination, but in which no visible vitrification has been induced.

This class of products is of very little value as cement. Numerous

experiments, made at different times and under a variety of circumstances, have established, almost beyond a doubt, that the genuine cements derived from the magnesian and argilo-magnesian limestones of this country, when they undergo any thing beyond a complete calcination, that is, when burnt any longer than is necessary to expel all their carbonic acid gas (and probably with some varieties somewhat below this point), lose in a great measure, if not entirely, their hydraulic energy; neither is it possible in practice to restore this energy by continuing the heat until the point of incipient vitrification is reached, as is claimed by some European writers. But as it is impossible to distinguish this stone from that which is suitably burnt, without a constant and tedious resort to the test with acid, which it would be found impracticable to enforce among workmen, it always, together with variable proportions of that which is insufficiently burnt, as well as that which is vitrified, forms a component part of the cement prepared for market. Suitably burnt cement contains a notable quantity of carbonic acid gas, and consequently effervesces, more or less freely, with hydrochloric acid. There appears to be no exception to this rule, although Mr. Vicat does not indorse it. The precise point at which the calcination should stop, or in other words, the exact proportion of carbonic acid gas which the stone should retain, in order to secure the most favorable results, has not been indicated with any degree of exactness, or in a way susceptible of practical application. It varies very much with the different kinds of stones, and particularly with the amount of silica and the alkalies which they contain. So great a difference indeed exists in this respect among the several layers of the same quarry, that any attempt to introduce and establish a mode of burning that shall be under sufficient control to secure uniform and constant results, under all circumstances, must contemplate a separate calcination for each of the dissimilar kinds of stone used in the combination. The idea, that stone which can be properly burnt in twenty-four hours can be mingled together in the same kiln with that which requires thirty-five or forty hours, to attain the same point, is as theoretically as it is practically untenable. Little or no extra expense would attend this process. The quarrying of the stone, it is true, would require a little more care and supervision, which might add one cent per barrel to the cost of the manufactured article, but this would be amply compensated by the amount of stone saved, to say nothing of the improvement in quality

secured thereby. Few of the manufacturers would require any additional kilns. The least extensive works keep from three to five in constant operation, with one or two in reserve, and there are but few quarries that would require a more extensive subdivision of their products than these would accommodate.

The several worthless products of the kiln noticed above, have their origin in a variety of causes, all of which, with proper precautions, are more or less under control; such as, variations in the force of the draft through the kiln, due to changes either in the direction and force of the wind, or in the barometric state of the atmosphere; neglecting to draw the burnt stone with the requisite care, taking perhaps equal quantities at stated times, which may be either too much, or not enough, depending on circumstances; not preserving the proper proportion between the fuel and raw stone, when adding these at the top; or not adding them at the proper time and in suitable quantities; irregularities in the settling of the stone in the kiln at each drawing, which results in some portions being exposed to the heat a much longer time than others; the formation of "cinders" or vitrified pieces of stone which adhere together or to the sides of the kiln, choking the draft and retarding the expulsion of the carbonic acid gas; these, and many other variable causes, will always operate to such an extent as to render the proper calcination of the cement, an operation of the utmost delicacy, and one requiring on the part of the manufacturer a high order of intelligence, experience, and skill.

Even the theory upon which this practice of mixing the fuel and stone together in the kiln avowedly rests, is singularly at fault, and will, by no means bear a critical examination; for, inasmuch as all the coal is consumed, or supposed to be consumed, during the calcination,—otherwise it is drawn with the cement and ground up in it,—and, as the proportion between the amount of fuel and raw stone, as well as the times of drawing the kilns, and the quantities drawn are also pre-established; and as no provision is made to regulate the force of the draft with a view to anticipate in a measure the intervention of one of the principal causes of variation referred to, it virtually assumes that a moderate heat, long continued, and a high heat proportionally short in duration, will produce identical results, a premise which, with all its apparent plausibility, is directly opposed to the teachings of experience.

Some varieties of argillaceous and argillo-magnesian limestone,

which yield a good cement with a certain degree of calcination, lose their hydraulic energy if the burning be carried beyond, or stopped short of that particular point. There are sometimes *two* points of maximum and *two* of minimum hydraulicity.

Take the argillaceous limestone from Lockport, New York, for example, of which a sample was sent me for trial. This was calcined at a bright red heat. Fragments were removed for trial at intervals of one hour, allowing nine hours for the last portions. All the burnings set rapidly in the air, but none of them sustained subsequent immersion in water, except the three corresponding to one half hour, two hours', and nine hours' calcination, respectively.

Similar trials were made with cement stone from other localities with varying results.

The curves of Diagram No. 1 will perhaps illustrate these peculiarities more prominently than a written description can. The numbers along the top denote the duration of the calcination in half hours. The central horizontal line, marked zero (0) indicates the point of hydraulic equilibrium, so to speak, at which the cements either part with or resume the power of "setting" under water, if immersed in the state of paste. In the vertical column of figures on the left, those above the zero line indicate the presence, and those below, the absence of this hydraulic power, the degree of intensity of these opposite characteristics being denoted by the magnitude of the figures in either case. The cements which furnish the curves were derived from the following localities:—

No. 1, from Lockport, New York, first specimen.
No. 2, " " " " second "
No. 3, " Centre of Round Top quarry, near Hancock, Md.
No. 4, " Stratum No. 15 from High Falls, Ulster county, New York.
No. 5, " Balcony Falls, Rockbridge county, Virginia.
No. 6, " Point aux Roches, Lake Champlain.
No. 7, " Stratum No. 7, Martin & Clearwater's quarry, Ulster county, New York.
No. 8, " Stratum No. 3, of the same quarry.

Observations on Diagram No. 1.—By examining the curves derived from the two specimens of Lockport cement, it is seen,

1st. That when burnt from one quarter to three quarters of an hour, both will set under water, and in combination would therefore make a good cement.

2d. Between three quarters of an hour and one and a half hour's calcination, the first will not set under water, while the second will, and the properties of a combination would depend on the proportion of the two adopted.

3d. Two hours' burning exactly reverses this state of things, the first setting under water, while the second will not, and this condition obtains until the calcination is continued for three and a half hours.

4th. Beyond this point, neither will set under water, until a calcination of seven and a half hours is reached, when the first becomes hydraulic, and continues so, the second remaining as before.

As there is no greater diversity among the foregoing eight varieties of cement than is ordinarily to be found in the several layers of the same quarry, which, according to the usual custom, are burnt together, we can to some extent realize, by an inspection of the diagram, the practical effect of the system now in vogue among manufacturers.

1st. All the eight varieties burnt from half to three quarters of an hour, set under water, and when thus treated, make a good combination.

2d. Calcined two hours, four of them set satisfactorily under water, and four do not.

3d. Burnt five hours, three of them set under water, and five do not.

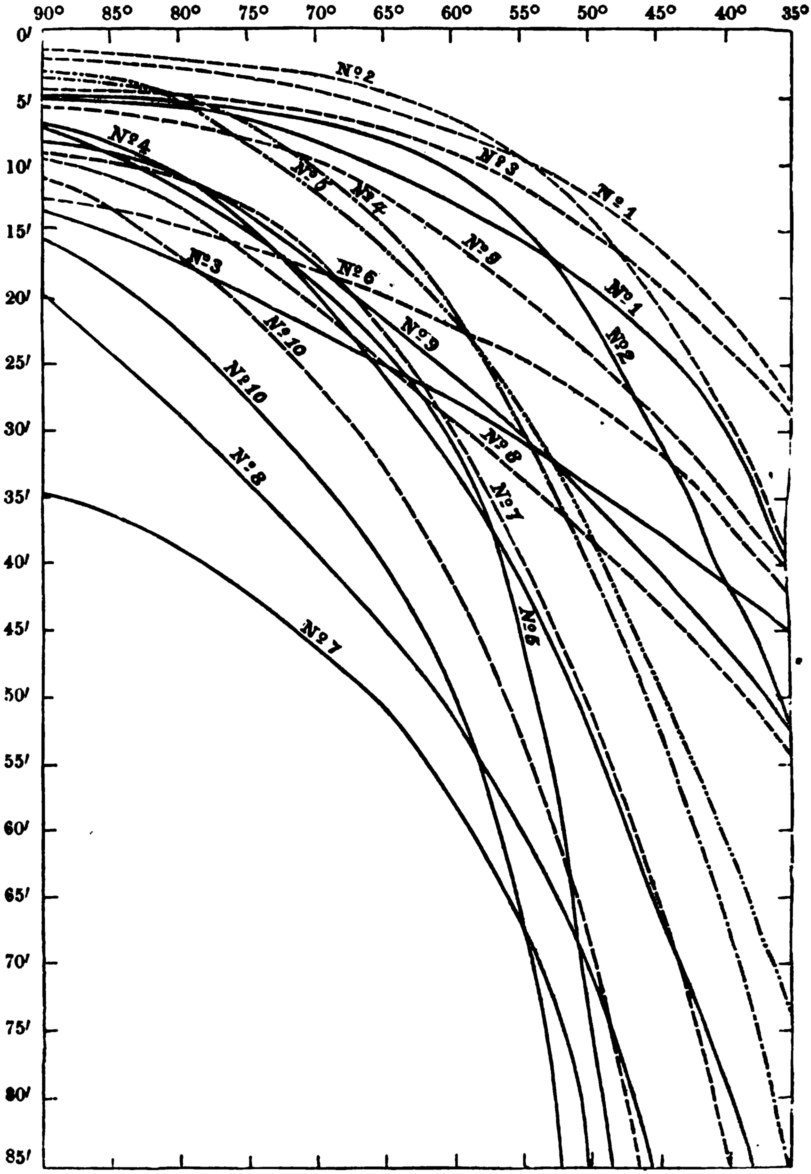
4th. Burnt six and a half hours, only two of them set under water (and even one of them rather sluggishly), and six do not. Three of these latter, however, may be regarded as hydraulic limes, and would indurate in time, while a fourth (stratum No. 15, from High Falls, Ulster county, New York), possesses all the worst features of the intermediate lime.

5th. Burnt eight hours, these specimens are again equally divided.

The scope of this paper renders impracticable any statement of the progress attained, or of the results developed, by the experiments now in progress. The following brief observations are submitted:

1st. The effect of a change in the temperature of the cement, and water of mixture, upon the time of setting, has been greatly underrated. A mortar is considered to have set, when it will support, without sensible depression, a wire $\frac{1}{12}''$ in diameter, loaded with quarter pound weight. At a temperature of 65° Fah., which was assumed for all the mixtures, the James River cement will set in five minutes, that from Hancock on the Potomac in six, the quick-setting Ulster county cements in twelve, and the slow-setting in thirty-five minutes.

Diagram I.


90°
85°
80°
75°
70°
65°
60°
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50°
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40°
35°
0′
5′
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65′
70′
75′
80′
85′
No 1
No 2
No 3
No 4
No 5
No 6
No 7
No 8
No 9
No 10

If the temperature be lowered 30°, the James River cement will require thirty-two minutes, the Hancock cement twenty-four minutes, the quick-setting cements from Ulster county forty-five minutes, and the slow-setting eighty-five minutes.

If, on the contrary, we *raise* the temperature 30°, the James River and Hancock cements will set in $2\frac{1}{2}$ minutes, and the quick and slow-setting cements from Ulster county in four and nine minutes respectively. The least difference in time of setting, in this variation of 60° in the temperature, is therefore $22\frac{1}{2}$ minutes, while the greatest difference is one hour and sixteen minutes.

Diagram No. 2, will show the effect of this variation of temperature, upon the hydraulic activity of cements. The curves are constructed with abscissas which represent the temperature of the air, water, and dry cement (these being varied correspondingly and kept together in all cases), and with ordinates which represent the times of setting in minutes, that is, the period of time which elapses before the immersed paste will support the loaded wire without depression. The dotted curves refer to the $\frac{1}{12}$ inch wire loaded to quarter of a pound, and the full curves to $\frac{1}{24}$ inch wire loaded to one pound.

The practical application of these deductions is quite evident. In cold weather (whenever it is necessary that the cement should harden quickly), warm water should be used for mixing the mortar, and wetting the solid materials with which it is to be used. In warm weather, on the contrary, cool water should be employed for the same purpose, in order to delay the setting until the mortar is laid in position.

2d. The time required by a cement to set (if within the ordinary limits of $\frac{1}{10}$ of an hour to $1\frac{1}{2}$ hours), furnishes no means of judging of the ultimate strength and hardness which it is likely to attain.

3d. It is not probable that while the present method of manufacturing cement is pursued in this country, we can produce an article equal to Parker's Roman cement, or the artificial Portland cement, from abroad.

4th. The stone furnishing what is generally termed *intermediate* lime, now rejected by our manufacturers, as worthless, on account of its containing an excess of caustic lime, may be used with entire safety, if combined with five to eight per cent. of an alkaline silicate. The compound silicate commonly called soluble glass is good for this purpose. By the same means, any requisite degree of hydraulic activity may be conferred upon mortars of common or ordinary hydraulic limes.

5th. The maximum adhesion to stone is secured by mixing the cement paste or mortar, very thin (*en coulis*), rather than very stiff. The maximum density, cohesion, and hardness, on the contrary, are all incompatible with this condition.

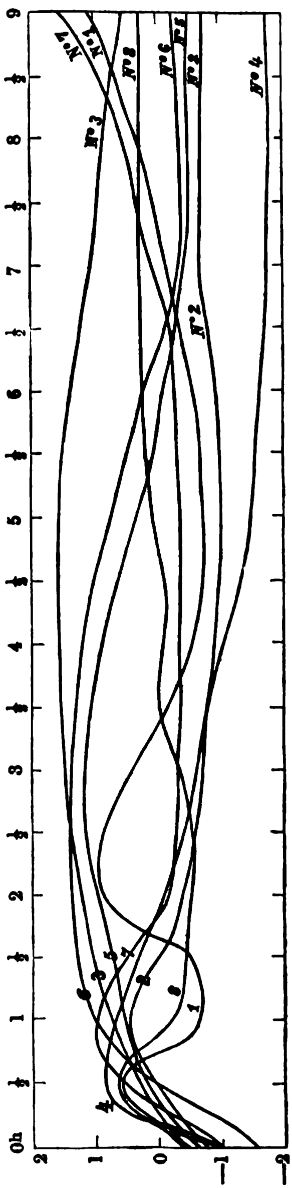

Diagram II.

6th. Cement should be ground to an impalpable powder, when it is intended to give mortar its full dose of sand, the coarse particles of cement being a poor substitute for that article.

Finally, all the stone which does not effervesce with dilute hydro-chloric acid, or which, during calcination, has been carried beyond the point of complete expulsion of carbonic acid gas, should be rejected.

Note. — Since the above was put in press, additional experiments render necessary some modification of the remarks on the *maxima* and *minima* of hydraulic energy. The following extract is made from a Report to the Chief Engineer of the War Department, now in course of preparation. Q. A. G.

Extract.

"With some varieties of stone, cements of inferior hydraulicity are yielded by a heat of moderate intensity and duration, at a stage but little in advance of a condition of incomplete calcination; with others, they are produced as we approximate to a state of incipient vitrifaction; with all, they are essential elements in the individual properties of

the stone, each quarry, and even the separate layers of the same quarry possessing distinct characteristic features in this respect, which features are, withal, subject to considerable variations within narrow lateral limits. The converse of these premises is also true, to wit, that the state of maximum energy corresponds to a condition of incomplete calcination in some cases, of complete calcination in others, while, in others still, it is only produced by vitrification more or less complete."

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2. Description of a new Portable Coffer-Dam. By Capt. E. B. Hunt, Corps of Engineers, U. S. A.

The use of the coffer-dam in laying foundations under water, is among the best established and most available resources of the engineering profession, and its application in several classes of cases is well settled. In making studies for certain contemplated constructions at Fort Taylor, Key West, a new style of coffer occurred to me, which I hope soon to apply, and which gives a rational promise of success.

The first case considered was one of founding wharf and bridge piers on a rock bottom, over which a thin stratum of sand is spread. A set of piers ten feet square of solid masonry from the bottom was first contemplated. For these the style of coffer planned was a strong, square frame, with four corner posts and a sufficient number of wale courses across the four sides and framed into these corner posts to give the stiffness of side-wall necessary for supporting the whole water pressure. The length of the corner pieces would be such as to give an excess of a foot or more at top in the deepest water at high tide. The size in plan would have to be such as to give the requisite working space, and might be reduced to fifteen feet square. This framework, being put together and stayed by a set of diagonal rope-tie braces, could be launched and taken to its position, where it would be placed erect and adjusted to the level, using, if necessary, uprights in one or more angles, to bear on the bottom or to be driven to the rock, and then lashed or bolted to the levelled frame. These angle posts can be sufficiently driven to give security against the force of tides and currents when needed, and also to sustain the weights required to be

rested on the top of the frame. The coffer frame, being thus fixed in position, a row of sheet piling of sound three inch hard pine plank remains to be driven to the rock, in contact with the wales and guided either by two outside timber guides, made to be removable or by fixed flat iron bar guides, with the angles smoothed.

Now comes the feature which I suppose to be entirely novel, and which gives a peculiar character to this portable coffer-dam. Take strong canvas and proceed to make up a case or covering for the entire coffer, using two thicknesses of canvas and interposing a complete coating of mineral or coal tar so as not only to cause the two canvas layers to adhere to each other thoroughly, but to make a perfectly impervious sheathing. Along the bottom edge of the coffer sheathing, a similar double canvas flap is joined around the whole bottom line, which will lay spread out over the bottom as far as is judged necessary. This breadth of flap will depend essentially on the nature of the bottom. The surface to be thus covered should first be raked clear of sticks, stones, etc., to prevent tearing holes through the flap. The case and flap, water-tight through their whole extent and having much positive strength to resist pressure, being put on over the coffer and surrounding bottom, it only remains to proceed with the pumping, which being actively pushed, will rapidly reduce the small enclosed water column. As this goes on, the exterior pressure comes first on the canvas coating, and this in turn rests against the sheeting piles and along the entire surface of the bottom. As the sheeting should be of even thickness with straight edges, the joints will be close and narrow; hence there will be no danger of ruptures from the bridging strain across them. The submerged exterior guides being either removed or formed of iron bars with bevelled edges would create no dangerous strains. To bring the flap more closely to the bottom, a sprinkling of sand or any clean earth might be thrown over it when in place, or the outer edge might be weighted if needful. In case the water should penetrate through the bottom covering layer even from the outer boundary of the flap, it is only required to scoop out the enclosed sand, and fill in the bottom with a layer of concrete as is usual in the common coffer, using the *tramis*, a plain wooden trough or a box with a trip bottom. It only remains to proceed in building the pier, using the top of the coffer as a platform, and to support the derrick or traveller, the materials being

lightered along-side. Should a steam pump be found necessary this could be worked on board a lighter, by using a flexible pipe, led through the side at top, or it could be carried through the case and sheathing near the bottom. A series of lashings along the top of the case could be used for fastening it, and buoyed cords attached to the edge of the flap would serve for its manœuvre.

Another mode of treating this case might be preferred for great depths. This is by using a circular coffer made by trimming to the required arc sweeps of three-inch planks, combining them in full circle ribs so as to break joints and fastening with screw bolts. This is merely turning an arch centre into the vertical. Launching one rib, a set of upright struts with draw bolts would be erected on it and the second rib built on them, &c. In some cases, this might be superior to the square coffer. The modifications of the case and flap would offer no serious difficulty. Various other timber and iron coffer frames might be advantageously used in treating this case.

In the instance first considered, it was desirable not to obstruct the waterway more than was necessary to get the solidity required by a permanent wharf for heavy vessels. A series of these piers giving the requisite supports for the wharf and bridge platforms, answered these conditions, and it was supposed that this plan could be used in water of over twenty feet.

The facility with which this portable coffer can be struck and re-established is its great recommendation. Admit the water, hoist the case, draw the sheeting and float the frame to its next station, buoying if necessary, and then all becomes simple repetition. It is a question of judgment or calculation in each case to give the framework the stability required for resisting the pressures, currents, and wave actions, as also to decide when the probable violence of waves would make the plan impracticable or injudicious. Judgment must also be used in deciding whether the ruggedness of the bottom makes this plan inapplicable. Sometimes this difficulty would be fully met by throwing around the coffer a covering sheet of fine clay or marl, which will either make the bottom tight or so cushion it that the flap can be used successfully. When we contrast the simplicity of this coffer and the facility with which it can be established and transferred, with the complex character of the ordinary fixed coffers, or with Stevenson's portable coffer, so limited comparatively in its applications and troublesome

in erection, it will need but little consideration to perceive the utility of this device in numerous cases of bridge-piers and other structures. To extend the above system to larger piers requires only the application of simple well-established principles, which every competent engineer would easily make, and need not be here dwelt upon.

It is likely to find its first application in a sea-wall which will probably be built at Fort Taylor next winter. It is proposed to use in this case a portable coffer of fifty by ten feet, in five compartments of framing, the intermediate submerged cross braces being made removable. The building of the first section of wall will not differ much from the building of a pier, except that the masonry bond at each end must be arranged to provide for the adjoining sections. In walls but little exposed to the sea, the sections can be brought above low-water independently by building plane heads and leaving a clear joint. Of course this would not do where the foundation is bad and the load irregular.

To build the second section, the coffer would be reëstablished as before, except that the end should be arranged to embrace the wall already carried up, and the sheeting should be shaped to close in neatly on its front and rear faces. The case and flap will have to be so altered at this end as to fit the section of the wall and extend along its front and rear faces for some distance. In the proposed wall, the use of dove-tailed header and streacher courses of granite is contemplated for the face and a massive concrete filling for the back. The box planking for the concrete can rest against the main uprights, and can be recovered on striking, thus leaving all the spare space in the coffer for face work.

The simplicity of this coffer and the facility with which it can be shifted from section to section of a sea-wall, lead me to believe that it will be found a great source of economy in constructing the walls of wharves, basins, docks, etc., when the shelter from waves and the character of bottom, make it available. In many cases, the flap could be nearly omitted, and in some rough bottoms the simple coffer case could be used, and a slight foot-slope of puddle or earth which works tight could be thrown in so as to serve the purpose.

This device not having yet been tried, I should scarcely bring it before the public except that I am willing, by publication at once, to prevent patents, and to give to engineers the benefits it offers, which can be seen beforehand with almost absolute certainty.

II. USEFUL INVENTION.

New Methods of preventing Fire-damp Explosions in Collieries, by Lighting with Coal Gas. By Capt. E. B. Hunt, Corps of Engineers, U. S. A.

Few chapters in the history of human industry are so appallingly tragic as that relating to explosions of fire-damp or carburetted hydrogen in collieries. Human experience, and even human imagination can scarcely add to the overwhelming terrors attendant on those fierce flashes of devouring flame, far down in the earth's deeps, where the feverish pulses of its ever-glowing central heat are felt. Instantly the rushing blast carries destruction not only to the costly machinery for moving and hoisting coal, but to the scattered groups of miners who, overwhelmed amid their labors, are crushed, stifled, or left to grope in almost hopeless darkness. This danger is not among things gone by, for we are still from time to time shocked by some fresh story of explosion, fraught with death to scores or even hundreds of poor victims. Despite these sadly frequent admonitions, the world has almost settled itself into the pleasing belief that Davy, by the invention of his safety-lamp, had made these explosions only possible through recklessness or stupidity. Such, however, is clearly not the fact. In chapters X. and XI. of Smiles's Life of George Stephenson, we find a striking narrative of the invention by Stephenson of his Geordy lamp, of the contest for priority between his and Davy's friends, and in conclusion some facts are cited which show how serious the danger still is. "In the eighteen years previous to the introduction of the lamp (Davy's safety), 447 persons lost their lives in the counties of Durham and Northumberland, whilst in the eighteen years following, the fatal accidents amounted to 538." The increased number of fatal accidents after Davy's lamp came into general use is ascribed in great measure to resuming work in many mines before abandoned as dangerous. Smiles says, "it must be admitted that the Davy and the Geordy lamps alike failed to stand the severe tests to which they were submitted by Dr. Pereira when examined before the Committee on Accidents in Mines," 1835. Dr. Pereira pronounces the Davy lamp "decidedly unsafe,"

when exposed to a current of explosive gas, and says that experiments have "demonstrated" the fallacy of the lecture room proofs of its safety. The above cited committee reported thus: "Accidents have occurred when his (Sir H. Davy's) lamp was in general and careful use; no one survived to tell the tale of how these occurrences took place; conjecture supplied the want of positive knowledge most unsatisfactorily; but incidents are recorded which prove what must follow unreasonable testing of the lamp; and your committee are constrained to believe that ignorance and a false reliance on its merits, in cases attended with unwarrantable risks, have led to disastrous consequences." Sir H. T. De LaBeche, in his London Exhibition Lecture of 1851, says: "The more effective saving of life from colliery explosions must be looked for in the instruction generally of the coal miners themselves. The amount of mischief arising from the foolhardiness of ignorance in our collieries, can only be credited by those who are compelled to employ men with a want of education they deplore, or who have in discharge of duties visited coal mines after fearful and desolating explosions."

I am aware of nothing since to change the foregoing state of facts, and certainly the stories of explosions still appear in the newspapers with formidable frequency. I regret being cut off from reference to more recent parliamentary investigations and statistics on this subject. It will scarcely be denied that additional security against fire-damp explosions remains a true desideratum. This must be my excuse for now bringing forward a plan which occurred to me while reading Stephenson's Life in the winter of 1858–9, and which seems to me not only to promise security to coal mines and miners, but to offer other valuable advantages over present methods. A distrust of my own acquaintance with the requirements of the case, has led me to keep silence hitherto, but really believing that the plan I shall propose has the essentials for practical success, I find that the reproach of *possible* sacrifice of life through reserve forbids me longer to suppress or hold back, for any reason, what *may be* the means of safety to thousands of fellow-beings. I shall not attempt to push explanation of this plan into minute detail, this being best left to those fully cognizant of the requirements and practice of coal mining.

To explain this plan, take the case of a mine worked from a single shaft. Erect on the ground near its mouth an establishment for manu-

facturing coal gas, purified for illumination, and add a gasometer. In this same vicinity a steam engine must be arranged for driving an air-condensing engine, the power being regulated by the labor to be imposed, as will be explained. Steam engines already existing for hoisting materials up the shaft can be used for this purpose, and in general one engine should do all the work of the mine. It is supposed, then, that all the coal gas and all the compressed air wanted is supplied at the mouth of the mine. Now the principle which I suppose to be new in this connection is simply this: The mine is to be lighted by coal gas supplied from the mouth, and the air for burning this gas is to be supplied from the outside atmosphere. None of the air of the mine must be admitted to the flame, and the products of combustion must only be set free at a safe distance from the flame, after passing through one or more wire-gauze screens. Thus perfect non-intercourse will be established between the flame and the air of the mine, and no fire-damp can reach the lights.

There are two distinct modes of carrying this plan into execution, the first of which I shall call the *portable* system, and the second the *fixed* system.

In the portable system, the gas and air are delivered through tubes down the shaft, at its bottom, where two moderate-sized reservoirs should be made by sealing excavations for the purpose, one to be kept filled with gas of high tension, and the other with strongly compressed air. It is now supposed that the lamp posts or burner stations are established throughout the whole route, to be kept permanently lighted. As this route is usually furnished with a railway transport from point to point along, it will be cheaply effected. Here two plans are available. In the first, which I suppose to be the best, a gas and air car would be used which has two reservoirs, made as large as the case admits, and in proper proportions. These reservoirs would be made of boiler plate with numerous cross-tie bolts, and would thus bear a great strain of the compressed coal gas and air, with a weight not objectionable. These reservoirs being filled, the car will proceed on its round of supply. Each lamp has its own reservoirs for gas and for air, which are quickly filled from the car, through flexible tubes, with stopcocks adapted to this object, and so on, in turn, will each burner be supplied.

The second plan of portable supply is but an application of the method to some extent in use in this country for supplying gas to

isolated residences, where gas pipes are not laid. Portable metal reservoirs are charged with gas and with air under high pressure, and distributed systematically to the burners, to which they are coupled before cutting off the exhausted set of air and gas reservoirs. Thus a perpetual light would be maintained, which would, however, require regulation to correspond to the tension of the gas and air. This could, probably, be readily effected by automatic regulators, governed by the pressures and in turn governing throttle or other valves. Water pressure regulation could also be well used sometimes. It might be found unnecessary to adopt any system of regulation, because of the cheapness of maintaining sufficient supplies in the reservoirs, and the experience acquired in using gas for lighting railroad trains would favor this expectation.

The burner and optical arrangements follow next in order. A simple tripod burner stand or column would receive the tubes supplying gas and air, and be otherwise wholly closed at bottom. Now it is clear that the principles applied by Fresnel in light-house illumination should here be followed. Regarding light as a value to be utilized, it is obvious that it should be reduced to horizontalism, and that the main portion of it should be thrown along the length of the gallery, much as in range lights. The combination of lenses and reflectors to serve these ends in the best manner, is a problem of practical optics readily resolved. A solid pressed-glass cage of lenses, with perhaps a single reflector, would thus be arranged to encase the burner without a single open joint. The lens mould once formed, the lenses would not be expensive, as they would not need to be large, the gas light itself being made small. This light would probably be best composed of a circle of jets supplied with air within and without. The economizing of air in this combustion presents a new study, but one which cannot fail, under experimental guidance, soon to reach a satisfactory result. The lens being closed at top, except a small orifice, the gaseous products of combustion would be led through a tube to some distance, and through several wire-gauze guards, a sponge, or other porous outlet. The elastic force of the supply will enable us to pass this stream through numerous fine gauzes, a bundle of wires, or other guards. We might even use valves.

A modification of this light would be required for use by the workmen. Reservoirs of India rubber small enough to throw over the

shoulders, with flexible induction and eduction tubes could be arranged to burn for a considerable time. The lens would be the smallest size of pressed glass lens, simply to horizontalize the rays or to give a beam on one or two sides. For working at the head of a drift, barrel-shaped reservoirs of the largest size, easily portable by hand, would be carried forward, and the gas and air drawn from them through flexible tubes to supply hand lenses, as in the preceding case. To light these several burners resort may be had to the electric spark, or to devices for enclosing friction matches, or to some percussion fixture. Ingenuity will readily solve this problem. For the fixed burners, the need of lighting will very rarely arise, as cleaning the inner lens surfaces and the burners will seldom be necessary on account of their seclusion from the air of the mine. I believe the fixed burners, the portable burners with temporarily fixed reservoirs, and the portable burners with bag reservoirs will meet all the needs of miners. In galleries not frequented as thoroughfares, and where lights carried on the coal trains will suffice, the fixed lights might be dispensed with. To save the head lights from extinction by stepping on the tubes, a core of spirally wound wire inside the impervious walls of the tube might be readily and cheaply made.

I now pass to the fixed system of gas lighting for mines. Returning to the bottom of the shaft, this system supposes gas and air mains to be led through the galleries to be lighted, and burners fixed where wanted, drawing their gas and air supplies directly from the mains, or from branch pipes in the usual manner. London, with its two thousand miles of gas mains and its radiation of gas from the Vauxhall works to Hampstead and Highgate, seven miles distant, must present actual solutions for nearly every case which can arise in arranging the pipes for mines. As only the galleries in use would need to be lighted, the length of pipe would be about double this gallery length, and the relative size of gas and air pipes would be fixed by their respective deliveries and tensions. Reservoir chambers for air and gas from point to point, so sealed as to exclude jets of carburetted hydrogen, etc., would be useful adjuncts in extensive mines. As fire could not be used in putting down the pipes, the joints might be made by giving a slightly conical form, and wrapping the enclosed end with strands of oakum dipped in mineral or coal tar, and then driving together. This would have another advantage, in the facility with which it would permit the shifting of

pipes from abandoned galleries to newly opened ones. The arrangement of burners, lenses, induction and eduction pipes offers no differences in principle from the portable system, except in the fact that the regulation is derived from the gasometer, and single stopcocks for each tube will suffice. In this system each light becomes perpetual, with no other need of supervision than an occasional cleaning. With these fixed lights will be combined the portables for the drift heads, drawing from the iron pipes through long flexible tubes or tubes with flexible joints, and the portable bag burners for emergencies.

There is an adjunct use which, on either system, can be made of the compressed air, though best on the fixed system. A locomotive driven by compressed air, from the reservoir at the bottom of the shaft, may be made to drive the coal trains, and thus remove horses from these mines, which, besides giving greater power at command, will help to purify the air. By establishing reservoir air chambers along the galleries, supplied with air through the air mains, the power of the air engine could be renewed, by fresh inspirations, at points where it might be needful. Often by closing sections of abandoned galleries, these reservoirs would be made of great capacity and at small cost. To this use of air could be added that of a mechanical ventilation of assured efficiency, by discharging air from the mains at the points of work. I suppose that this will be found the proper application of all that power otherwise wasted, but that this will not supersede existing arrangements for automatic ventilation. I venture the suggestion that compressed air being thus conveyed to the working points, would soon find its application as a power with which to do much of the heavy work of digging, and all of the loading, thus effecting a great saving of labor and increase of production.

This quiet transfer into the bowels of the earth of an effective power, obedient to the slightest touch, will open a new field for invention. The application already made of this agency in tunnelling the Alps, as described to the French Academy by Col. Manabrea of the Sardinian Engineers, is a clear demonstration in practice of the use of compressed air as a medium for conveying mechanical power, which I had the privilege of discussing before this body at its first Albany meeting in 1851. When we bear in mind that both coal gas and mechanical power will be produced with the maximum possible cheapness at the mouths of bituminous coal shafts, we see a beautiful natural provision

for their application as now indicated. Not only will life find increased security in the anticipated exemption from explosions, but by better ventilation and the diminished number of miners day's labor to a given quantity of coal mined. The substitution of a brilliant gas light for the dim, caged, smoky oil lamps now used, will not only facilitate mining labors, but will give some color of good cheer to these dingy realms. Not least of all will be the fact that this systematic arrangement for gas lighting will be given into a few intelligent hands, while the more ignorant ones, whose mental darkness De La Beche so justly deplores, will never be permitted to touch a light, and will thus be cut off from the too dangerous power of risking the lives of all their comrades. In cases of choke damp or carbonic acid floods, the use of the air mains for expelling by steam power the unwelcome intruder is obvious, and may be important.

It is evident that as a preliminary, a careful course of experiments should be made to determine with accuracy the conditions for effective combustion under the circumstances named. Should it be found practicable, with safety and without too great loss of light, to burn the gas and air when previously mixed, in the proper proportions, a single pipe might be made to replace the two parallel pipes, through which the mixed air and gas should be supplied. The danger of explosive actions in this mixture, and the loss of incandescence of the flame by mixture, are likely to be fatal to this simplification. Prof. Rood has suggested the use of gauze screens in the tubes to arrest explosions. It would be inconsistent with the fundamental idea of this system to admit any risks of explosions in the lighting apparatus; hence it seems requisite to adhere to the double pipe system as explained. To complete either system will require a direct application of mechanical and optical principles, for which a perfectly attainable combination of knowledge, ingenuity, and judgment would be fully equal, with but little groping or experience of first failures.

The use of compressed air for mechanical purposes in mines would constitute a separate question of the first magnitude. I cannot doubt but this truly philosophical system of transporting power from the best point for power manufacture to the desired point of application, will here find a most happy exemplification. The mode of action which is alike equal to driving the great tunnelling machine of the Sardinian Alps or a hundred sewing machines in a New York shirt factory, will

be found not less strong or subtle when applied to the cyclopean labors of coal mining, whether in wielding the pick and hammer, in hoisting the black nuggets on the cars, or in the more accustomed labors of locomotion. There must, moreover, be numerous cases in mining anthracite and other minerals beyond the direct access of steam engines, where the power manufactured in the open air and converted into the form of compressed air can with great advantage be led down into the depths to do the work now exacted at great cost from human muscles. This method of operation would in many cases be found to afford a far more efficient ventilation than has ever yet been reached in galleries closed at one extremity. The free use of transferred power for working and for transport, would doubtless make many mines profitable sources of mineral supply, which now yield but meagre returns, or are hopelessly relinquished. Time and experience can alone test in full the efficiency of the projects now stated, but their rational nature and legitimate promise can be at once appreciated by minds possessed of the requisite furniture of knowledge, judgment, and vision.

TITLES

OF

COMMUNICATIONS.*

A. MATHEMATICS AND PHYSICS.

1. On some of the Relations of the Violet and Green Modifications of Chrome Alum to Soda, Potassa, and Ammonia. By E. N. Horsford.

2. The Great Auroral Display of August 28 and September 2, 1859. By Elias Loomis.

3. Brief Abstract of a Memoir on the Theoretical Determination of the Dimensions of Donati's Comet. By W. A. Norton.

4. On Natural Ice-Houses and on Frozen Wells. By Elias Loomis.

5. Influence of Difference in the Mean Velocity of Winds from the Different Points of the Compass, in Modifying the Mean Direction of the Atmospheric Currents over the United States. By James H. Coffin.

6. Can the Sudden Cooling of one Part of a Metallic Rod cause another Part, as a Consequence, suddenly to become warm. By E. N. Horsford.

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* The following papers were also read: of some, no copy has been received for publication; of others, it was voted that the title only should be printed. No notice, *even by title*, is taken of articles not approved.

7. On the Data and Methods of the Hindu Astronomy. By W. D. Whitney.

8. On Atmospherical Electricity. By Joseph Henry.

9. On a Series of new Combinations of Ammonia, Picric Acid, and Metallic Bases. By M. C. Lea. Presented by B. Silliman, Jr.

10. On certain Variable Stars. By B. A. Gould.

11. Modern Warfare; its Science and Art. By E. B. Hunt.

12. On the Actinism of the Electric Discharge in Vacuum Tubes. By W. B. Rogers.

13. On the Hydrates of Sesquioxide of Chromium. By E. N. Horsford.

14. On a Series of Investigations on the Assimilation of Gaseous Nitrogen of Plants, conducted during the Years 1857, 1858, and 1859, at Rothamsted, England. By Evan Pugh.

15. Vital Statistics of the Blind, with an approximate Life Table. By E. B. Elliott.

16. On the Meteors of Aug. 11, 1859, and July 20, 1860. By B. A. Gould.

17. On Induction-Time in Electro-Magnets. By A. D. Bache and J. E. Hilgard.

18. The Loss by Fire of Berdan's Mechanical Bakery in Boston, due to Spontaneous Combustion. By E. N. Horsford.

19. On the Motions of Uranus. By Truman H. Safford.

20. The Meteor of July 20th, as seen from Washington, Dutchess County, New York. By James Hyatt.

21. On Certain Variable Stars. By B. A. Gould.

22. A new Ammonia Chrome Alum, and the Violet, Green, and Red Modifications of Chrome Salt. By E. N. Horsford.

23. Systematic Views on Mechanics and Mechanism, with Inferences on Organisms. By E. B. Hunt.

24. On the Nitric Acid and Ammonia in Rain Water, collected September, 1859, between New York and Liverpool about the Middle Line of the Atlantic Ocean. By Evan Pugh.

25. On the Relations of Salts of Zinc and Alumina to Soda and Potassa. By E. N. Horsford.

26. Analysis of a Bituminous Earth from Brazil. By E. N. Horsford.

B. NATURAL HISTORY.

27. Jottings on the Geology of the Eastern part of Maine, etc. By W. B. Rogers.

28. On the Recent Discovery, by Mr. Norman Eastop, of Fossils in the Conglomerate of Taunton River. By W. B. Rogers.

29. On the Origin and Stratigraphical Relations of the Trappean Rocks of Lake Superior. By J. W. Foster and J. D. Whitney.

30. On the Lead Region of the Upper Mississippi. By J. D. Whitney.

31. On the Arrangement of the Museum of Comparative Zoölogy at Cambridge. By Louis Agassiz.

32. On the Origin and Distribution of the Sediments composing the Stratified Rocks of North America. By J. S. Newberry.

33. On the Surface Geology of Western America. By J. S. Newberry.

34. Some Points in the Surface Geology of the North-west. By J. D. Whitney.

35. Remarks on the Distribution of Gold in Veins, with Notes upon Remarkable Gold Specimens from Georgia. By W. P. Blake.

36. On the Ethnological Value of the Imitative Faculty in Relation to the Characteristics of Ancient and Modern American Races. By Daniel Wilson.

37. On the Petroleum Wells of the Mississippi Valley. By J. S. Newberry.

38. On Certain Phenomena of the Great Dismal Swamp in Virginia and North Carolina. By Nathan B. Webster.

39. On Methods in Zoölogy. By Louis Agassiz.

40. On the Food of Birds, with Special Results in the Investigation of the Food of the Robin (Turdus Migratorius). By J. W. P. Jenks.

41. On the Later Extinct Floras of North America. By J. S. Newberry.

42. Age of the So-called Taconic Rocks in Vermont. By C. H. Hitchcock.

43. On the Origin of the Prairies of the North-west. By J. D. Whitney.

EXECUTIVE PROCEEDINGS

OF

THE NEWPORT MEETING, 1860.

HISTORY OF THE MEETING.

THE Fourteenth Meeting of the American Association for the Advancement of Science was held at Newport, Rhode Island, commencing on Wednesday, August 1, and continuing to Wednesday noon, August 8.

The number of names registered in the book of members in attendance on this meeting is one hundred and thirty-five. Thirty new members were chosen, of whom all but eight have already accepted their appointment, paying their assessment and most of them signing the constitution. Seven others have joined the Association by virtue of Rule 2 or 3. A few others have paid, without a formal election, or signing the constitution. Eighty-four papers were presented; most of which were read, but only a part have been printed. Some were thought unworthy of publication, and, in other cases, copies have not been furnished by their authors.

The sessions of the Association were held in the capitol of the State. The Vice-President of the last year, Prof. Edward Hitchcock, introduced the President of the Newport Meeting, Isaac Lea, LL. D., who took the chair.

The session was then opened by a prayer from Rev. A. H. Dumont, D. D. The Mayor of Newport, Hon. William H. Cranston, welcomed the members of the Association, in the following Address:—

MR. PRESIDENT AND GENTLEMEN OF THE ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—In behalf of the authorities and the people of Newport, I cordially welcome you to our pleasant Island City by the sea. You visit us at an interesting season of the year, when you will have an opportunity to see what has been done by nature and art to make this city a desirable place of residence; you will also undoubtedly become acquainted with many distinguished persons from various parts of the world, and I am confident that you will long remember the occasion with pleasure as one of the marked eras in your existence.

You are assembled upon soil which is consecrated by hallowed memories. It was in Rhode Island, more than two hundred years ago, that Roger Williams, John Clark, and their associates first thoroughly, practically, and successfully illustrated to the world the great and glorious doctrine of religious toleration—entire freedom for man to worship God, in an unmolested manner, according to the dictates of his own conscience. For ages before, distinguished theologians and reformers had vainly endeavored to secure to the human family that which was finally accomplished, by the great and good men whom I have named, in this small section of the western world; from that time until this day the people of Rhode Island have carefully cherished and guarded this sacred right of man.

In the fearful and trying days of the Revolution, the people of Rhode Island, having been loyal as long as loyalty was a virtue, and obedient to the government of the mother country until its oppressive demands could no longer be endured,—arose in their power and triumphantly vindicated their rights which had so long been trampled upon with impunity. They gave to the common cause of freedom a brave, calm, determined, and patriotic defender of the inalienable rights of man,—their noble Green, who was second only to the father of his country. In the last war with Great Britain, this city furnished her gallant and heroic Perry, who, with his associates, very many of whom were from this city, so nobly and successfully fought the sanguinary battle of Lake Erie.

A short distance from where you are now assembled, at Whitehall, Bishop Berkley lived several years, and on the rocks by yonder beach he wrote his "Minute Philosopher." In the venerable Redwood Library, which is stored with ancient and valuable works of science, art, and literature, Dr. Channing "studied theology without an instructor."

It was in this city that Stuart, Malbone, and Allston lived and labored in the art to which their genius and their lives were devoted. Previous to the Revolutionary war, the Jews were among the most wealthy, enterprising, and valuable citizens of Newport. The Synagogue, which still stands in Touro street, in excellent condition, was the first which was erected in the United States, and worship was regularly held there until long after the Revolution, when the Jews all removed to other cities where their mercantile pursuits could be more advantageously prosecuted. Abraham Touro gave $10,000 for the repairs of the Synagogue; from the surplus interest of this fund the granite and iron fence around the lot was constructed; he also gave $5,000 for the repairs of the street which bears his name. Judah Touro erected the substantial fence around the Cemetery, at a cost of about $12,000; he also gave $10,000 for the purchase of the Park, which bears his name, and in which is located the "Old Stone Mill," a mysterious relic of a by-gone age; he bequeathed $3,000 to the Redwood Library Association, and $10,000 as a Ministerial and Cemetery Fund, for the purpose of paying the salary of a reader or minister to officiate in the Synagogue in this city, and to endow the ministry of the same as well as to keep in repair and embellish the Jewish Cemetery. In addition to these he made, during his life, at different periods, valuable donations to the city. He was the last of a noble race of men who were liberal to all, and especially to the city of Newport, where they once lived, and which they always loved. In yonder cemetery, consecrated by Jewish rites, sacredly guarded by Christian care, repose the mortal remains of the families of Touro, Lopez, Polock, Riveara, Hart, Franci, Seixas, Hays, Isaacs, Judah, and others. While their memories are lovingly cherished by our people, their earthly forms will unmolestedly remain in that beautiful cemetery until the Messiahship is completely fulfilled.

The formation of our soil and its rock-bound coast afford an interesting study for geologists; and the beautiful location of our city, its pleasing and varied scenery, healthy climate and genial air, especially at this season of the year, make it a delightful resort, not only for pleasure-seekers and invalids, but for men of science, literature, and art, for recreation after their severe mental employment during a considerable portion of the year.

The building in which you are now assembled was erected one hundred and twenty years ago, and from that time to the present the gov-

ernment of the State has been annually inaugurated in this room; here, also, have the tribunals of justice held their terms in administering the law. These walls have frequently echoed with the eloquence of Webster and Choate and many other distinguished advocates, and the wisdom of Story and other learned jurists, very many of whom have passed to the final rest of man on earth. In the adjoining Senate Chamber is the original portrait of our beloved Washington, which was painted by Stuart. There are many other interesting associations connected with the history of Newport which I might enumerate if time permitted; but you will undoubtedly be made acquainted with many of them during your sojourn here.

The object of your Association is the advancement of science, — the utmost development of the intellectual faculties with which our race is endowed, so that man can not only understand the alphabet of the Creator of the Universe, but also, in a great measure, comprehend the language of His Omnipotence as written in the soil and the rocks, on the ocean, in the skies, and on and in every material element of His vast creation. By the progress of science, by the gradual development of the mind in consequence of continuous and earnest study, devoted astronomers have explained to the human family that they need not be alarmed at an eclipse of any of the heavenly bodies, or the appearance of comets or other unusual objects in the skies. From the geologist and the chemist agriculturists have received and are constantly receiving valuable information which enables them to cultivate the soil more easily and successfully; and those who navigate the oceans are yearly acquiring inestimable knowledge by the efforts of scientific men. All departments of human industry, all classes of society, in all parts of the world, are being rapidly benefited by the efforts of the members of such associations as the one which I now have the honor of addressing. Science is a noble study; by it we do not read the productions of the intellect of a human being, as a learned and fascinating book in literature, and behold not a beautiful picture or a noble statue, as in art; but we read the eloquent language and behold the gorgeous paintings and august statues which emanated from the Creator when He had finished His work, and "saw every thing that He had made, and that it was very good," and rested from His labor on the seventh day. His sublime language is indelibly engraved on all portions of the foundation, and everlastingly illuminated over the broad canopy of His universe.

Religion and science are linked in a holy and inseparable union; for who can behold the wonderful works of the Creator, and study their nature and character, and yet deny His omnipotence? Every thing in nature speaks of His divinity; and the true man, the man of soul, intellect and faith, contemplates the mysterious works of God with reverential and child-like devotion, and he seeks, not only for his own instruction and gratification, but for that of his fellow men also, to know God in His works as far as our finite capacity will permit. Thus, through and by nature he looks up to nature's God, and in a measure approaches the primeval condition of our first parents. Pope said: —

"Lo, the poor Indian! whose untutored mind
Sees God in clouds, or hears him in the wind;
His soul proud science never taught to stray
Far as the solar walk, or milky way;
Yet simple nature to his hope has given,
Behind the cloud-topp'd hill, an humbler heaven;
Some safer world, in depth of woods embraced,
Some happier island in the watery waste,
Where slaves once more their native land behold,
No fiends torment, no Christians thirst for gold:
To be, contents his natural desire,
He asks no angel's wing, no seraph's fire;
But thinks, admitted to that equal sky,
His faithful dog shall bear him company."

There is a fascination in the faith and philosophy of the aborigines, and a simple yet touching eloquence in their character; but it is *our* blessing to live in an age when men are not satisfied to grope on in darkness, and live only as their ancestors lived; there is a genius in man's nature which is progressive, a divinely created element which, by proper action and careful cultivation, incites his aspiration for a closer communion with, and a more perfect knowledge of, the infinite Being from whom he came, and to whom he must return.

"The soul of man
Createth its own destiny of power;
And, as the trial is intenser here,
His being hath a nobler strength in heaven."

I have the pleasure, Mr. President, of presenting to you, in behalf of the authorities of the city, copies of a Road Map of the Island of

Rhode Island, with geological delineations, for the use of the members of the Association.

I again cordially welcome you, Mr. President and gentlemen of the Association, to our ancient city, and trust that, while your deliberations will undoubtedly materially benefit the human race, your social intercourse with our citizens and visitors will be so pleasant, you will ever remember the occasion with emotions of joy, and return to us again at some future period.

President Lea responded in these words:—

Gentlemen of the American Association for the Advancement of Science.—It would be very difficult for me to express to you how sensible I am of the honor you have conferred on me in appointing me to preside over this, the Fourteenth Annual Meeting. In my humble pursuits, and my circumscribed efforts in the cause of science, I have labored without any ambition of such distinction, and must crave your indulgence, as I am sure of your coöperation and your courtesy to each other, in any want of experience in presiding over large bodies, where a knowledge of parliamentary rules and usages is necessarily requisite. I have, however, the most entire confidence in your indulgence and assistance, and in the harmony which has always existed among us; and I promise you that I shall have but one object in view, that of the strictest impartiality in presiding here. It must be the source of the highest gratification to each of you present, as well as to every member absent, that our Association has continued to hold its meetings with such unvarying success from year to year, forming a concentrated congress, as it does, of a large portion of the best science of the country, from the North and the South, and the East and the West. Coming together, as we do, to a great Exchange of science, to give and to receive, we return to our closets with accumulated scientific wealth, which, unlike all other exchanges, takes nothing from any individual, while each one carries treasures away. And now it becomes my painful duty to state that the year since our last meeting has not passed without its bringing the record of loss to the science of our country. We have been deprived by death of a colaborer in James P. Espy, in his 75th year. He had done much to promote a knowledge of American Meteorology, to which branch of science he had given his undivided attention for many years.

The President concluded by thanking the Mayor and city authorities for their welcome.

On Thursday evening, August 2, Professor Joseph Henry delivered a lecture on Atmospheric Electricity, in Aquidneck Hall.

On Saturday evening, August 4, Professor A. D. Bache delivered a lecture on the Gulf Stream, in Aquidneck Hall.

On Wednesday, August 8, the address of the retiring President was given at the place of general session, by Professor Stephen Alexander.

No lengthened abstract of the proceedings, scientific and executive, of the Newport Meeting of the Association, is necessary in this place, as they are contained in full in the papers and resolutions printed in this volume.

The Association voted to hold their next meeting at Nashville, Tennessee, on Wednesday, the 17th of April, 1861.

The officers elected for the next meeting are, Pres. F. A. P. BARNARD, of Oxford, Mississippi, President; Professor R. W. GIBBES, of Columbia, S. C., Vice-President; Professor W. P. TROWBRIDGE, of Washington, D. C., General Secretary; and Dr. A. L. ELWYN, of Philadelphia, Treasurer. The Permanent Secretary, Professor JOSEPH LOVERING, of Cambridge, was elected for a fourth term, commencing with the Nashville Meeting.

During the session of the Association, the members and their ladies, besides receiving many private hospitalities, were elegantly entertained on Friday evening, August 3, by Daniel Parish, Esq.; and on Monday afternoon, August 6, by Mrs. Dr. Howe, at Lawton's Valley. On Saturday afternoon, an excursion was made, by invitation of Capt. G. W. Cullum, of the U. S. Engineers, to Fort Adams. Preparatory to this excursion, an interesting paper, "on Modern Warfare: its Science and Art," had been read in the morning, by Capt. E. B. Hunt, who was also present at the fort to impart valuable information in regard to its construction and objects.

RESOLUTIONS ADOPTED.

Resolved, That the retiring President be requested to communicate to the proper Sub-Committee of the Standing Committee, within three months after the adjournment of the Association, whether he will be prepared to deliver a retiring address at the next annual meeting: and if not, that the Vice-President shall be expected to perform that duty.

Resolved, That Dr. B. A. Gould be requested to make a report, at the next annual meeting, on the History and Present State of the Application of Telegraphic Methods to Astronomical Observation, and the Determination of Longitudes.

Resolved, That Prof. W. B. Rogers be requested to make a report, at the next annual meeting, on the Present State of the Theory of Binocular Vision.

Resolved, That Prof. Newberry be requested to make a report, at the next annual meeting, on the Fossil Botany of the United States.

Resolved, That the series of Fossil Plants from the Brown Coal Formation, presented to the Association by Hon. Elisha Dyer, of Providence, R. I., be referred to Dr. Newberry for examination.

Resolved, That the Committee on Weights, Measures, and Coinage, are authorized to correspond with such departments of the government of the United States, with the Commissioners to the Statistical Congress, and such other persons at home or abroad as they may deem expedient, to obtain information to be laid before this Association in furtherance of the objects of their appointment.

Resolved, That the memoir of Gen. J. G. Totten "on the Disappearance of the Ice of our Northern Lakes" (a copy of which was not received in season to be printed in the last volume), be printed in the volume of Proceedings at Newport.

VOTES OF THANKS.

Resolved, That the American Association for the Advancement of Science returns thanks to the Legislature of the State of Rhode Island for the use of the State House for the meetings of the Association.

Resolved, That the thanks of the Association be returned to the Sheriff and other State officers for their earnest and successful endeavors to afford increased accommodations and facilities.

Resolved, That the thanks of the Association be returned to the Mayor and Corporation of the city of Newport, for the cordial welcome which they have extended to its members.

Resolved, That the Association returns thanks to the Directors of the Redwood Library, and of the Newport Reading Room, for their kind invitation, throwing open their institutions to the use of the members.

Resolved, That the thanks of the Association be returned to Capt. Cullum of the United States Engineers, for his most cordial invitation to visit Fort Adams, and to Capt. Hunt of the same Corps for his agreeable reception, and for his instructive lecture on the defences and modes of attack.

Resolved, That the thanks of the Association be given to the citizens and residents of Newport, who have contributed greatly to the enjoyment of the members.

Resolved, That the Association returns thanks to the Directors of the Railroad and Steamboat lines which have offered its members a free return passage.

Resolved, That the thanks of the Association be returned to the Secretary of the Local Committee, Samuel Powel, Esq., for the efficient aid which he has rendered the officers of the Association in the general conduct of the meeting.

Resolved, That the thanks of the Association are due to the President, Vice-President, and General Secretary, for their efficient and impartial discharge of the duties of their respective offices.

Resolved, That the thanks of the Association be returned to Prof. Stephen Alexander, for the eloquent and philosophic address with which he has favored us.

REPORT OF THE PERMANENT SECRETARY.

In accordance with the rule of the Constitution the Permanent Secretary submits the following annual report: —

The various duties discharged during the past year are the same as for former years; and as they have been carefully described in previous reports, it is not necessary, perhaps, to repeat them now. On account of the difficulty of finding any gentleman to discharge the duties of Local Secretary at Newport, it has been necessary for the Permanent Secretary to assume his duties, so far as was consistent with his residence at a distance, and to issue the circular for the Newport Meeting. In company with Dr. Wolcott Gibbs he visited Newport in April, and with the coöperation of Dr. David King obtained the use of the State House for the use of the Association; which was granted by a special act of the Legislature. The Mayor also was visited, and interest and sympathy awakened in him towards the objects of the Association.

The Association now contains 862 members; seventy-five new members were added at Springfield, and 247 were then struck off, on account of delinquencies in the payment of assessments: their aggregate indebtedness to the Association being two thousand two hundred and twenty-three dollars.

The financial condition of the Association is as follows: —

Between August 3d, 1859 (the first day of the Springfield Meeting), and August 1, 1860 (the first day of the Newport Meeting), the income has amounted to fourteen hundred and ninety-nine dollars and forty-three cents ($1,499.43); of which eighty dollars ($80) came from the sale of the volumes of Proceedings, and the balance from the annual assessments.

The expenses of the Association during the same time have been seventeen hundred and thirty-two dollars ($1,732), which may be classified, in general, thus: —

Expenses connected with the publication of the Springfield volume, one thousand dollars and fourteen cents,	$1,000.14
Expenses connected with the Springfield meeting, one hundred and twenty dollars and fifty cents,	120.50

Expenses connected with the Newport meeting, thirty-five dollars,	35.00
Salary of the Permanent Secretary, five hundred dollars,	500.00
Postage and other expenses, seventy-six dollars and thirty-six cents,	76.36

The items may all be found in the cash account of the undersigned, which is herewith submitted as a part of his report. The balance in the hands of the Permanent Secretary has diminished, since the last account, by one hundred and ninety-eight dollars and eighty-seven cents ($198.87). The balance in the hands of the Treasurer is five hundred dollars ($500).

JOSEPH LOVERING,
Permanent Secretary.

NEWPORT, August 1, 1860.

CASH ACCOUNT OF THE

Dr. AMERICAN ASSOCIATION in

Express to Springfield,	$2.00
Clerk at Springfield,	25.00
Gas bill at Hampden Hall,	3.50
Chauncey Wright for services on programmes,	15.00
Grant & Warren for paper,	319.20
Smithsonian Institution, for freight,	2.50
Committee on Chili expedition,	12.00
C. F. Loosey, for freight,	3.28
Kilbourn & Mallory, for wood-cuts,	21.25
J. Bien, for printing plates,	76.25
Allen & Farnham, for printing Springfield Proceedings,	456.68
Bradley, Dayton & Co., for binding do.,	78.94
Salary of Permanent Secretary,	500.00
Journey, etc., to Newport, in April,	21.00
Printing circulars,	12.50
F. B. Hough's bill for committee,	5.00
Binding 250 copies of Prof. Caswell's address,	1.50
Freight on foreign distribution,	2.00
Buck's express,	33.54
Allowed Permanent Secretary for attending Springfield meeting,	75.00
Index to the volume of Springfield Proceedings,	5.00
Stationery of Secretaries,	10.00
Postage and discount on assessments,	36.86
Postage on circulars for Newport meeting,	9.00
Clerk for directing Newport circulars,	5.00
	$1,732.00
Balance to next account,	389.98
	$2,121.98

PERMANENT SECRETARY.

Account with JOSEPH LOVERING.	Cr.
Balance from last account, ...	$588.85
Check from the Treasurer, A. L. Elwyn, ...	33.70
Assessments, etc. (from No. 477 to No. 878 of Cash Book),	1,499.43
	$2,121.98

STOCK ACCOUNT OF THE PERMANENT SECRETARY.

Volumes Distributed or Sold.

VOLUMES	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	XIII.
DELIVERED TO													
Boston City Library,													*
Nantucket Athenæum, †													
Albany State Library,													*
Phil. Acad. Nat. Science, †													*
Phil. Philos. Soc., †													*
Providence Athenæum, †													*
Brown University, †													*
Yale College, †													*
Harvard College, †													*
American Academy, †													*
Boston Nat. Hist. Society, †													*
Boston Athenæum,													*
Montreal Nat. Hist. Society, †													*
Super't Education, Montreal, †													*
Smithsonian Institution, †													*
Lyceum of Nat. Hist. N. Y., †													*
Maryland Institute, †													*
Maryland Historical Society, †													*
Redwood Library, in Newport,													*
Mayor of Newport,													*
Mayor of Springfield,													*
Sold or exchanged,	2	3	3	2	3	2	2	3	4	6	3	5	14
To Members,							1	1	1	1	7	33	287
Total,	2	3	3	2	3	2	3	4	5	7	10	38	331

List of European Institutions to which Copies of Volume XIII. of the Proceedings of the American Association were distributed by the Permanent Secretary in 1860.

Stockholm, — Kongliga Svenska Vetenskaps Akademien.
Copenhagen, — Kongel. danske Vidensk. Selskab.
Moscow, — Société Impériale des Naturalistes.
St. Petersburg, — Académie Impériale des Sciences.
" " Kais. Russ. Mineralogische Gesellschaft.

* Sold.
† By order of the Association.

Amsterdam, — Académie Royale des Sciences.
Haarlem, — Hollandsche Maatschappij der Wettenschappen.
Berlin, — K. P. Akademie der Wissenschaften.
Breslau, — K. L. C. Akademie der Naturforschen.
Franckfurt, — Senckenbergische Naturforschende Gesellschaft.
Freiberg, — Königlich Sächsische Bergakademie.
Leipsic, — Königlich Sächische Gesellschaft der Wissenschaften.
Göttingen, — Königlich Gesellschaft der Wissenschaften.
Munich, — K. B. Akademie der Wissenschaften.
Prag, — K. Böhm Gesellschaft der Wissenschaften.
Stuttgart, — Verein für Vaterländische Naturkunde.
Vienna, — K. Akademie der Wissenschaften.
" K. K. Geographischen Gesellschaft.
" Geologischen Reichsanstalt.
Bern, — Allgemeine Schweizerische Gesellschaft.
Basel, — Naturforschende Gesellschaft.
Geneve, — Société de Physique et d'Histoire Naturelle.
Neuchatel, — Société des Sciences Naturelles.
Bruxelles, — Académie Royale des Sciences, &c.
Liège, — Société Royale des Sciences.
Lille, — Société Nationale des Sciences, de l'Agriculture et des Arts.
Paris, — Institut de France.
" Société Philomatique.
Dijon, — Académie des Sciences, &c.
Turin, — Accademia Reale delle Scienzie.
Madrid, — Real Academia de Ciencias.
Cambridge, — Cambridge Philosophical Society.
Dublin, — Royal Irish Academy.
Edinburgh, — Royal Society.
London, — Board of Admiralty.
" East India Company.
" Museum of Practical Geology.
" Royal Society.

BALANCE OF STOCK.

Volumes	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	XIII.
Balance March, 1860,	58	93	253	235	439	299	582	854	957	897	852	1010	
Received from Binders,													1435
Received from others,*	3	1					1	1	1				
Total,	61	94	253	235	439	299	583	855	958	897	852	1010	1435
Delivered as on page 242,	2	3	3	2	3	2	3	4	5	7	10	38	321
Balance,	59	91	250	233	436	297	580	851	953	890	842	972	1114
Delivered as on page 242–243,													38
Balance March, 1861,	59	91	250	233	436	297	580	851	953	890	842	972	1076

REPORT OF THE AUDITORS.

This certifies that we have this day examined the above account of the Permanent Secretary, comparing the credits with the Treasurer's account and with the receipt-book, and the debits with the several vouchers, and find the whole correct, and the sum of three hundred and eighty-nine dollars and ninety-eight cents ($389.98) credited in the next account.

(Signed) John LeConte,
B. A. Gould.

Newport, August 6, 1860.

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* In exchange for volumes less rare.

REPORTS.

The Committee of Weights and Measures and Coinage, considering that the movements recently made in these subjects are of sufficient importance to draw the attention of the Association to them, and desiring some extension of their powers, respectfully present the following report of progress: —

The mission of Professor J. H. Alexander not having led so far to any arrangement in regard to international coinage, this subject still remains an open one, with the disadvantage that the British Commissioners have reported against the decimalization of the Imperial coinage. Commissioners have been sent by our government, under an appropriation from the last Congress, to the Statistical Congress at London, and with them as well as with our government, it is desirable this committee should have authority to correspond.

The recent presentation of one of the bronze standard yards, prepared by the British commission, to the office of Weights and Measures of the United States, has permitted a new comparison of the Troughton scale, which constitutes the United States standard of length, with the new British Imperial standard yard. This gives the United States standard yard 0.000,873 of an inch longer than the British yard.

Sir John Herschel, writing in the Athenæum, says, "If the British Imperial standard inch were increased by one thousandth part, it would be with all but mathematical precision, one five hundred millionth part of the earth's axis of rotation." If the standard temperature for the United States yard, which has never been authoritatively here fixed, be taken at 59°.8 Fahr. instead of at 32°, the "modular standard" will be exactly the 500,500,000dth part of the earth's polar axis.

A very remarkable report on weights and measures has been presented to the American Pharmaceutical Association by Alfred B. Taylor, Esq., of Philadelphia, to which the Committee would earnestly call the attention of the members of the Association.

After a thorough and interesting discussion of the several methods hitherto proposed for adapting our manifold systems of weights and measures to the decimal scale, it suggests still other and perhaps better methods of adaptation, whether the grain Troy, the pound Avoird., or

the pint wine measure, be taken as a standard. An ingenious, and not unpractical system is also presented, derived from the "new foot," so called, this being the one fifth part of the length of Leslie's seconds-rod, and differing by only about one fourth of an inch from the present standard foot. Starting from this unit, consistent and correlative tables for linear and for cubic measure and for weight are derived, in which none of the standards are far removed from those now in popular use.

But throughout the discussion, the guiding idea which serves as one premise for all conclusions is this, that "all experience shows, that however much more rational it may be to assign new terms to new things or ideas, mankind is ever far more ready to adopt a change of substance than of form, and far more prone to transfer old names to new but analogous uses, than to accept an unfamiliar nomenclature expressly contrived for such new uses." Unquestionably there is force in this idea; although hardly so much as to warrant us in assuming the contrary to be impossible.

The conclusions at which the report arrives are; that the decimal scale ought to be rejected in any new system of weights and measures, on account of the impossibility of halving the subdivisions of the number *ten*; and they proceed to offer for adoption a new system, founded upon the octonary notation, for which they propose new digits and new names, in spite of the fact that the procedure is not entirely in accordance with the principle just laid down.

Strong as this reasoning is, it seems to us hardly forcible enough to outweigh the great facts that the numeration of the civilized world is and must unquestionably ever remain a denary notation; that half the currency of the world is a decimal currency; that the tendency of those nations which do not now enjoy the advantage of a coinage and currency, in harmony with their numeration, is setting strongly towards its adoption; that even in England, the most conservative of civilized nations, efforts to no insignificant extent have for several years been making to decimalize the national currency; that however erroneous its units may theoretically be, still the elegance, simplicity, and harmonious relations of the French decimal system of weights and measures have already insured its almost universal adoption for scientific purposes in all countries, until even the established units of weight, force, heat, in almost every department of science and art are dependent upon the gramme, the meter, and the degree centigrade.

The number (8) has been selected by the author of the report for the basis of his proposed systems, as being at once a number of convenient size, and a power of two; the principle of successive halvings being regarded by him as the fundamental one of all human subdivision. It is conceded by him that one octonary system of weights and measures should imply one octonary currency, yet he regards the change to such a system in the former case, even without a modification of the currency, as likely to be attended by decided and important benefits.

But whatsoever may be our views regarding the recommendations of the Committee, the report is one of great value and instructiveness, and for the thorough discussion and richness of the material presented may be placed in the same rank with the classic report of the late John Quincy Adams.

By order of the Committee,
A. D. BACHE, *Chairman*.

The Committee appointed at the Baltimore meeting of this body to determine an abbreviation of the name of the Association would respectfully submit the following report: —

It is agreed that the words "American," "Association," and "Science," should be represented in an abbreviation, as defining the locality, organic character, and object of this body. To include the word "Advancement," would give inconvenient length, and is thought unnecessary.

In abbreviating "American," as "A." is in extensive use for "Academy," the "m" must be added, making the common form "*Am.*" Astronomy has a prior right to "*As.*," and it is proposed to guard against ambiguity by adding the terminal letter on a raised line or in the form of an index, thus, "*As*^{n}." The word "Science" is usually and well abbreviated to its first syllable "*Sci.*" The total abbreviation of the name of this body therefore becomes "AM. AS^{N}. SCI."

To indicate its published volumes, these are denominated "Proceedings," which is abbreviated to "*Pr.*" The particular volume will be indicated by the year of meeting following the general abbreviation, thus: PR. AM. AS^{N}. SCI. 1860.

Should it be desired to add the name of the place of meeting, as

will be necessary in the cases of the volumes for the semi-annular meetings, this can be done by the use of the first syllable of the name in parenthesis, thus: "PR. AM. ASN. SCI. 1860 (NEW.), p. 88."

The Committee would recommend the introduction of the proper abbreviation on the title page of each volume, in the belief that a general adoption of this plan in the publications of scientific societies and journals or magazines of science would be found exceedingly convenient, and would soon fix the usage in citation. It should however be remarked, that these abbreviations have need to be systematically framed, to avoid ambiguity and inconvenience.

(Signed) E. B. HUNT,
WILLIAM B. ROGERS,
S. L. LOOMIS.

The Committee on Dr. I. I. Hayes' proposed Expedition to the North Pole, report that they have coöperated with Dr. Hayes in procuring means and instruments for the proposed expedition; that the outfit obtained for Dr. Hayes being deemed by him adequate, and the instruments provided being sufficient for the purposes of the exploration, the party sailed from Boston in the schooner United States on the 7th of July of the present year.

The Committee asks to be continued.

(Signed) A. D. BACHE, *Chairman.*

NEWPORT, R. I., August 7, 1860.

CORRESPONDENCE.

MAYOR'S OFFICE, NEWPORT, R. I., May 9, 1860.

MY DEAR SIR, — Your kind letter of the 7th was received this afternoon, and two copies of the 13th Annual Report of the Proceedings of the American Association, etc., I received this morning, for which please accept my thanks. I will send the copy for the Redwood Library to that Association. Do you think that your Association would prefer to hold their first meeting in August in a larger hall than that of the State House, where the regular meetings are to be held during the session? If so, I should like to be informed of the

fact, as you can have a larger hall which will accommodate about one thousand persons, whereas the hall in the State House will not seat probably more than four hundred or five hundred. If you can give me any information in regard to this matter, it will afford me much pleasure to make the necessary arrangements.

Yours, very truly,

WILLIAM H. CRANSTON, *Mayor*.

Professor JOSEPH LOVERING, Cambridge, Mass.

P. S. I speak of the *first* meeting, as I suppose that will be numerously attended, perhaps more so than the others.

MAYOR'S OFFICE, NEWPORT, R. I., June 14, 1860.

MY DEAR SIR, — Your favor was duly received. The Legislature of our State, at its session in May, passed a Resolution placing the State House in this city at the service of your Association for your meeting in August next. Our City Council, at its last meeting, passed the enclosed Resolution.

Yours, very respectfully,

WILLIAM H. CRANSTON, *Mayor*.

Prof. JOSEPH LOVERING, *Secretary, &c.*

At a meeting of the City Council of the City of Newport, holden June 12th, A. D. 1860.

Resolved, That his honor the Mayor, together with Alderman John C. Ailman and Common Councilmen Robert J. Taylor, Thomas Coggeshall, and John T. Bush be, and they are hereby appointed, a committee to coöperate with the local committee of the American Association for the Advancement of Science, in making arrangements for the Annual meeting of said Association in this city, in August next.

A true copy — Attest,

BENJAMIN B. HOWLAND, *City Clerk*.

STATE OF RHODE ISLAND, ETC., SECRETARY'S OFFICE,
PROVIDENCE, July 20, A. D. 1860.

DEAR SIR, — A letter received from Newport a few days since has led me to think that the American Association for the Advancement of Science did not understand that the State House in Newport had been

placed at their disposal for the next meeting, which has induced me to communicate to you the following resolution of the General Assembly, passed in June last:

Resolved, That His Excellency the Governor be, and he is hereby authorized and requested to tender to the American Association for the Advancement of Science, the use of the State House, in Newport, as a place for the meeting of the convention of said Association to be holden in Newport in the month of August next.

I perceive that it has been publicly announced that the Association will meet at the State House in Newport in August, so that there can be no mistake in relation to it.

I have directed the sheriff of Newport county to have the building in readiness for the Association.

Yours, very respectfully,

JOHN R. BARTLETT, *Secretary*.

Prof. JOSEPH LOVERING, Cambridge, Mass.

REDWOOD LIBRARY AND ATHENÆUM, NEWPORT, R. I., June 4, 1860.

DEAR SIR, — I beg to inform you that our Board of Directors at their meeting in May last, passed a resolution requesting me in their behalf to offer to "the American Association for the Advancement of Science" the use of our library and reading room, and of the other rooms, for Committees, during their session to be held in this place in August next.

It gives me great pleasure to communicate this offer, and I trust it will not be unacceptable to the Association.

I forward this letter to you as the Permanent Secretary of the Association, and, I presume, the appropriate officer to receive such communications, and am,

With great respect, your obedient servant,

GEORGE G. KING, *President Redwood Library, &c.*

Prof. JOSEPH LOVERING, SEC. AM. ASN., ETC., Cambridge, Mass.

The Governing Committee of the Newport Reading Room beg to tender the freedom of their rooms to the members of the Scientific Association, during their stay at Newport.

In behalf of the Committee,

J. D. OGDEN.

NEWPORT, August 1, 1860.

RAILROAD ARRANGEMENTS.

NEWPORT, R. I., July 22, 1860.

To the President or Local Superintendents.

DEAR SIR, — On behalf of The American Association for the Advancement of Science, I append a list of several Railroad Companies, who were so liberal last year as to extend to the members of "The American Association for the Advancement of Science" the right of free return tickets after the close of the session at Springfield.

May I beg to know whether your line will this year grant the same facility to the members returning from the Newport meeting, which is to begin on the first of August.

Your reply, if favorable, will be communicated to the members when assembled here. I have the honor to be,

Yours, very respectfully,

——— ——— *Local Secretary.*

New Haven, New London, and Stonington	Railroad.
Philadelphia, Wilmington, and Baltimore	"
Boston and Worcester	"
Connecticut River	"
Western	"
Michigan Southern	"
Hartford and New Haven	"
New York Central	"
Cincinnati, Hamilton, and Dayton	"
Connecticut and Passumpsic River	"
Vermont Valley	"
Vermont and Canada	"
Cape Cod	"
Richmond and Petersburg	"
Pacific	"
Pittsfield and North Adams	"
Troy and Saratoga	"
Amherst and Belchertown	"
Saratoga and Whitehall	"
Sullivan	"
Michigan Central	"
Vermont Central	"
Fitchburg and Worcester	"

CERTIFICATE

On Behalf of the American Association for the Advancement of Science.

To the Superintendents, Ticket Agents, and Conductors of the several Steamboat and Railroad Companies enumerated below.

[NOT TRANSFERABLE.]

The Presidents or Superintendents of the several Companies mentioned below having in writing signified to the Local Secretary, that members of the American Association for the Advancement of Science, who have attended the Newport meeting, will be passed back upon their return free of charge, upon conditions that such members shall have passed over the said roads on their way to the meeting.

Now, therefore, this Certifies that ______________ has attended the meeting at Newport, held August 1, 1860, and upon the subsequent days, and that he certifies, over his own signature hereon, at the bottom, that he has, in coming to the meeting, passed over and paid fare upon those lines not stricken out of the list below.

NEWPORT, R. I., August , 1860.

(Signed) JOSEPH LOVERING, *Permanent Secretary.*
SAMUEL POWELL, *Local Secretary.*

The Philadelphia, Wilmington, and Baltimore Railroad.
" Vermont Valley "
" Cape Cod "
" Old Colony and Fall River "
" Rensselær and Saratoga "
" Amherst, Belchertown, and Palmer "
" Connecticut River "
" Bay State Steamboat Company "

I hereby certify that I have passed over and paid fare upon all the lines above enumerated and not stricken out, in coming to the meeting of the American Association for the Advancement of Science, held at Newport, August 1st, etc.

NEWPORT, R. I., August 3, 1860.

INDEX.

N.

O.

P.

R.

S.

T.

ERRATA.

On page 5, line 4 from top, for *pholosphere* read *photosphere*.

On page 37, line 6 from top, for *o* read *of*.

On page 101, this paper of Mr. Lea was not read or presented, and was sent by mistake.